Monitoring key indicators is a critical step in adaptive management.

5 Elements of Adaptive Management

Certain interlinked elements are necessary for managing adaptively. These include: defining problem boundaries, identifying key questions, generating alternative hypotheses about system function, designing rigorous experiments, monitoring, and then using the information to adjust activities and objectives (i.e., "feedback"). Defining measurable management objectives is a critical antecedent to effective adaptive management. Applying these elements with creativity and imagination is integral to dealing effectively with uncertainty and change.

". . . an essential feature of dealing adaptively with uncertainty is to reject recipes and rituals in favour of a search for better processes to promote imagination and learning" (*Walters 1986*).

5.1 Defining problem boundaries

Role

Problem bounding makes the complex problem of forest management tractable.

How do we do it?

It is critical to define a problem in terms of its structure, rather than in terms of preconceived solutions. By defining a problem in terms of preconceived solutions, we limit ourselves to learning only whether or not those *particular* solutions work.

"... there are no natural boundaries for defining renewable natural resource systems or the limits of management responsibility in dealing with them…the domain of potential concerns becomes a matter of practicality and continuing adaptation" (*Walters 1986*).

Defining problem structure focuses attention on how a system functions, which in turn can lead to creative management solutions.

Both Holling (1978) and Walters (1986) discuss problem-bounding in the context of developing a computer model that simulates the dynamics of a system (see Section 6.1). The following ideas (summarized from Walters 1986) apply equally well, however, when a computer model is not used as a tool.

Boundaries can be defined in four dimensions:

- the breadth of factors considered (e.g., timber production, biodiversity, site productivity);
- the depth of detail;
- the spatial scale and resolution (e.g., stand, landscape, region); and
- the time scale and resolution (e.g., 20 years, one rotation, 500 years).

Problems can be bounded effectively by working outwards from a few key indicators of system performance, to include factors that influence the variables identified, but stopping where further detail becomes impractical or unnecessary. It is important to keep in mind that there are no natural problem boundaries, only practical ones. It is therefore often necessary to modify the problem boundaries as modelling proceeds. In fact, Walters (1986) advocates deliberately viewing the problem from a number of different angles, suggesting that the resulting "jolts" in perspective can stimulate new ideas and creative ways of tackling the problem.

While it is important to avoid getting bogged down in unnecessary complexity, it is equally important to avoid defining a problem too narrowly. Defining problems too narrowly limits the range of management options that can be explored, and may result in the omission of factors critical to management decisions and outcomes. In particular, cumulative effects, emergent properties, and interactions between scales may be missed. Kessler et al. (1992) emphasize that "detailed knowledge about constituent parts, and about individual resource responses, may not add up to understanding of the system." By defining problems too narrowly, we also run the risk of investing in questions that turn out to be relatively insignificant for making management decisions.

"In adaptive policy design it is essential to begin by rejecting the intuitive notion that learning is always valuable, and to instead view the resolution of uncertainty as an important step only insofar as it may help to improve long-term management performance . . ." (*Walters 1986*).

5.2 Identifying key questions

Role

It is important to: (i) identify those uncertainties about system function that have implications for management, and (ii) identify those uncertainties that *can* be addressed by manipulative management experiments.

There are numerous uncertainties about how forest ecosystems function. One key component of adaptive management is to differentiate those uncertainties that affect management decisions from those that do not. Management experiments should focus on resolving uncertainties that will yield information about how to manage more effectively. Specifically, they are valuable when: (i) there is a high level of uncertainty about which of several management alternatives will best meet objectives, (ii) alternative explanations for an observation suggest different management prescriptions, and (iii) different management prescriptions are predicted to have different impacts (Walters 1986).

(i) The level of uncertainty is high — The value of information gained from a management experiment may more than offset its cost if the level of uncertainty is high. Conversely, there is little need to focus on questions for which the answer is relatively certain. In such cases, implementing and monitoring the one "best" treatment (i.e., passive adaptive management) may be more appropriate.

(ii) Alternative explanations for a particular observation suggest different management strategies — Discriminating between alternative hypotheses about ecosystem function can help identify the most effective management regime (e.g., Walters et al. 1992; Semel and Sherman 1993; Sainsbury et al. 1994). Conversely, a deliberate management experiment is unwarranted if alternative hypotheses suggest the same management solution, even if there is a high level of uncertainty about which hypothesis is correct. According to Walters (1986), "the existence of large uncertainty about system response need not imply that the best action is also uncertain."

(iii) The ecosystem or performance indicator is sensitive to changes in management activities — Under these conditions, different prescriptions would have different outcomes. Conversely, a management experiment to resolve uncertainty is unwarranted if different management actions would produce similar responses.

The importance for management of resolving a particular uncertainty can be quantified by calculating the "expected value of information" (Walters 1986; Marcot 1989; McAllister and Peterman 1992a). Management experiments should focus on those questions where the expected value of information is high (i.e., where the three conditions noted above are true).

How do we identify key uncertainties?

A systematic process is needed for identifying uncertainties, and distinguishing those that will affect management decisions (the "need-to-know questions") from those that will not (the "nice-to-know questions"). Individual questions addressed in management experiments must be linked to one another, and to the overall vision defined by ecological sustainability and social values. For example, managers must consider not only how to provide buffers that are windfirm, but also whether providing windfirm buffers contributes measurably to management objectives. We also need a means for distinguishing those questions that can be addressed in deliberate management experiments from those that are best addressed by other methods, such as descriptive studies or detailed basic research.

We could start by reviewing the FPC for uncertainties, and articulating the biological assumptions that underlie it. Articulating the assumptions underlying overall goals may highlight additional uncertainties. The significance of these uncertainties for management decisions must then be assessed. This can be done informally, based on the experience of managers, or through a formal, explicit process (e.g., Richey et al. 1985). It is vital to integrate separate questions, relating them to overall goals, and linking questions at different spatial and temporal scales, and at different levels in the ecological hierarchy.

Another means for identifying key uncertainties is to develop a simulation model to explore the response of key indicators to management alternatives (Holling 1978; ESSA 1982; Walters 1986; Walters and Holling 1990). The process of model-building highlights uncertainties and provides a context for assessing their importance relative to one another and to the overall management issue (Lee 1993; Volkman and

McConnaha 1993). Those that must be resolved in order to build the model or that affect predicted outcomes are highlighted; others are discarded. When the model is developed in a workshop, time limitations force participants to distinguish those questions they "need to know" answers to from those it would be simply "nice to know" answers to (Holling 1978; Walters 1986). The potential benefits and problems with modelling workshops (AEAM workshops) are discussed in Section 6.1.

Any process for identifying key questions should include a range of participants to ensure that there is a range of perspectives, and that questions are indeed relevant to both management decisions and overall goals.

5.3 Developing alternative hypotheses

Once the key questions have been identified, they are usually phrased as hypotheses — statements that express a belief about the way things are. In classical statistical analysis, hypotheses typically are phrased as "null hypotheses" that posit "no effect" (e.g., "corridors are not useful features in landscapes"). Researchers then attempt to reject or disprove the null hypothesis, thus showing that an effect indeed exists (e.g., "corridors are useful after all"). For making management decisions, however, we need to know more than simply whether a treatment results in a particular effect. Managers also need to know the *magnitude* of a response to a management activity, the response over a *range* of conditions, or the *reason* for a particular response. For example, managers need to know how many corridors to maintain and how wide they should be. Explicitly stating and then testing a range of alternative hypotheses helps managers to understand the relationship between a treatment (e.g., corridors) and an indicator.

While it is a trivial task to generate testable hypotheses, it requires careful thought to define the few that are most relevant to management decisions. Given that the set of alternative hypotheses determines the design of the monitoring scheme and thus the value of information gained, more time and effort need to be dedicated to their construction than has typically occurred in the past. Developing alternative hypotheses is a creative activity. Managers can use past experience, local knowledge, and data from other places or situations, or they can develop generalities from specific observations (induction) to generate plausible, testable hypotheses about system function and response to management activities.

5.4 Experimental design

Three potentially complementary approaches to developing and testing hypotheses include:

1. classical statistical approaches, where hypotheses are tested through classic experimental design and frequentist statistics (i.e., concerned with random plot designs, sampling designs, Type I and II errors, confidence, power);
2. non-classical statistical approaches, where hypotheses are tested using Bayesian approaches, or where information from a variety of sources is formally combined (Draper et al. 1992); and
3. hermeneutics (Apel 1972), where self-consistent stories are constructed to explain ecological systems and provide a framework for interpreting new information (e.g., see Maser and Trappe 1984).

CLASSICAL APPROACHES TO EXPERIMENTAL DESIGN AND STATISTICAL ANALYSIS

Role

Rigorous experimental design is important for discriminating between alternative hypotheses and elucidating cause-and-effect relationships between management activities and observed outcomes. Experiments that are poorly designed produce results that at best are ambiguous or uninformative, and at worst are misleading.

What is involved in designing good experiments?

Experimental design, together with statistical analysis, allow us to address the question: "did the management activity cause the observed change?" The basic elements of classical experimental design are the same for all experiments, including management experiments. These are briefly outlined below (based on Hurlbert 1984):

1. use of *null* and *alternative hypotheses*;
2. *controls* against which to compare one or more treatments units;
3. *replicates* of treatment and control units, in time and space, to control for random variation; and
4. allocation of treatments in space and in time to control for *bias* and *environmental gradients*, and to ensure *statistical independence* of treatments.

In addition to these four elements, statistical power and significance levels must also be considered when designing management experiments. Detailed discussion of experimental design and its application to ecological experiments is presented in Hurlbert (1984), Hairston (1989), and Krebs (1989). Appendix 4 presents a brief discussion of the application of the elements of experimental design to large-scale experiments in forest management.

NON-CLASSICAL APPROACHES

Many problems in forest management cannot be phrased and tested in accordance with classical statistical analysis. In particular, classical statistics is not well suited to analyzing responses to large-scale perturbations where replication is often impractical or impossible (Carpenter 1990; Reckhow 1990). While we may be able to replicate treatments at a small scale, extrapolating the results to the large scale at which many management actions occur can be controversial and uncertain (Likens 1985).

Classical statistics is suited to answering questions about whether or not a treatment caused a response (i.e., whether to reject or not reject the null hypothesis). However, for making management decisions, we are often more interested in the *magnitude* of a response (i.e., the degree to which the null hypothesis is false, or the probability that alternative hypotheses are true). For example, we are not just interested in whether or not forest interior habitat is necessary for maintaining biological diversity, we are interested in *how much* forest interior is necessary. Although power analysis, which is part of classical statistics, quantifies the probability that an experiment will reject the null hypothesis, and determines the size of an effect that can be detected, it still does not directly answer the question about the degree to which a hypothesis is false. Rephrasing questions to make them more amenable to classical statistical analysis may make the answers less relevant for making management decisions.

Fortunately, alternative methods of statistical analysis, such as Bayesian statistics, are available for analyzing results of management experiments. Bayesian inference can be used:

- to analyze data from experiments where replication is impractical or impossible (i.e., because of the scale of the treatment and response) (Carpenter 1990; Parma and Deriso 1990; Reckhow 1990);
- to consider alternative hypotheses, and calculate the degree to which each is true; and
- to compare the informativeness and expected value of alternative management regimes and experimental designs (as part of formal quantitative decision analysis) (e.g., McAllister and Peterman 1992a; Sainsbury et al. 1994).

A brief explanation of Bayesian statistics, including some of its disadvantages, is provided in Appendix 4.

OTHER SOURCES OF INFORMATION — WHAT TO DO WHEN WE CANNOT IMPLEMENT POWERFUL EXPERIMENTS

Although well-designed management experiments are perhaps the most powerful way of discriminating between alternative hypotheses and clarifying whether an action caused an effect (Romesburg 1981; Walters and Holling 1990; McAllister and Peterman 1992b), it is sometimes impossible or impractical to design powerful experiments at an operational scale, in an operational setting. For example, it is difficult to envision a controlled, replicated experiment that addresses the effects of global warming. Sometimes managers will have to adopt a "passive" approach to adaptive management. With passive approaches, the manager evaluates existing information and implements the policy that is "best," assuming that the most likely hypotheses about ecosystem function is indeed correct. Outcomes are monitored and compared to predictions and pre-treatment conditions.

Managers can draw on a number of sources of information to help them in both identifying the most likely hypotheses and best policy, and in interpreting outcomes. Useful sources of information include:

- results from research on ecosystem processes;
- extrapolation of results from treatments that are applied at a small scale (i.e., where control and replication are possible);
- descriptive or observational studies;
- retrospective studies of past management activities;
- "natural experiments" (involving observations of natural variability, rather than deliberate perturbation);
- local knowledge and lived experience of First Nations and others; and
- expert opinion.

While information from the above sources may not provide direct evidence of cause and effect, it may provide a large body of circumstantial evidence sufficient to support or reject a particular management activity (Diamond 1986; Walters and Holling 1990). It can increase the level of comfort managers have in management decisions and predictions about outcomes, although some uncertainty will remain.

Descriptive and retrospective studies can also provide information on natural variability and baseline conditions that is important in designing and interpreting manipulative experiments.

Hermeneutics, where self-consistent stories are constructed to explain ecological systems (e.g., Maser and Trappe 1984) can provide a framework for making predictions and interpreting the outcomes of management activities. Formal techniques for combining information from a variety of sources (Draper et al. 1992) may also prove useful in adaptive management.

MAKING TRADE-OFFS

Given "real-world" constraints faced by managers, it will rarely, if ever, be possible to design "ideal" management experiments. Designing effective and economic experiments will inevitably require trade-offs and compromises; some rules of experimental design may have to be relaxed. Information comes at a cost. For example, increasing the number of replicates, the intensity of monitoring, or the length of the experiment may increase the informativeness of the experiment, but may also increase the costs of monitoring and the amount of revenue foregone. Because not all experimental designs are equally informative or equally expensive, it will be useful to explicitly identify trade-offs, and quantitatively compare alternative designs (see Section 6.2). McAllister and Peterman (1992b) suggest a number of measures for comparing designs, including: (i) the probability of correctly discriminating between alternative hypotheses, (ii) the expected economic value of alternative designs, and (iii) risk.

5.5 Monitoring

Role

Monitoring can be defined as the repeated observations, through time, of aspects of the ecosystem to determine the state of the system (Clayoquot Sound Scientific Panel 1995). Typically, most monitoring in resource management has focused on: (i) ensuring that policies are implemented as intended and comply with regulations, and (ii) detecting changes in the ecosystem. However, to improve forest management, we must do more than improve compliance and document changes. Monitoring when done in conjunction with good experimental design and appropriate data analysis, can allow managers:

- to determine whether practices are meeting objectives;
- to improve understanding of the mechanisms that underlie ecosystem function and change (i.e., to test alternative hypotheses);
- to determine the effect of management actions on the ecosystem; and
- to identify thresholds and anticipate shifts in the state of the ecosystem.

Information gained through monitoring must be fed back into the planning process to inform future decisions and effect changes in management.

How do we do it?

For some issues, monitoring must be carried out at a variety of spatial scales, from broad regions to specific sites. This will require that monitoring schemes be designed at a range of scales. For example, monitoring schemes applied at the local or site level may have to be designed at the regional level. There must be sufficient measurements and adequate interspersion of sample points at each scale to allow integration of information between scales. Monitoring will also have to encompass a variety of temporal scales, since the time required to detect the effects of activities may range from several months to several rotations (i.e., hundreds of years).

The effort required to monitor effectively varies enormously, depending on the activity. Often, there is a trade-off between the detail and precision of measurements and the number of measurements possible. Because detailed measurements are often expensive, they may be limited to a few sites and occasions, whereas less precise (and cheaper) measurements can be done more frequently, at more sites. Some combination of extensive and intensive monitoring is probably ideal, and would allow the periodic calibration of coarse measurements against more precise measurements (Clayoquot Sound Scientific Panel 1995).

In order to separate the effects of management activities from changes due to natural variability, it is necessary to collect data from both control and treatment units, before and after the treatments are applied. Inventories done during the initial stages of planning can serve as a baseline against which future states are compared; these inventory measurements may then be repeated after management activities are initiated. Where appropriate, common standards for data collection, such as those emerging from the Resources Inventory Committee (RIC), should be adopted.

Monitoring programs should focus on the key indicators that will tell us how well we have reached ecological, social, and economic objectives. For example, indicators of watershed integrity may include: the number of slope failures and volume of soil displaced per unit time, runoff, and water quality (Clayoquot Sound Scientific Panel 1995). Examples of social and economic indicators could include: harvesting costs, job creation, and the flow of a range of commodities derived from the forest.

Monitoring may measure the present *state* (e.g., species composition) of various aspects of the ecosystem, and it may measure *processes* (e.g., water flow). Because processes give rise to states, and states influence processes, measuring changes in processes may allow us to anticipate changes in states. Indicators (whether states or processes) should be:

- sensitive to the treatment being applied;
- reliable and specific (i.e., able to differentiate effect of treatment from other effects);
- cost effective to measure; and
- relevant to objectives (Noss 1990; Whitfield et al. 1992).

Indicators may provide direct or indirect measures of how well management activities meet objectives. For example, for determining the effects of management activities on the maintenance of biological diversity, the Clayoquot Sound Scientific Panel (1995) suggest monitoring fragmentation, edge effects, and other detrimental factors (i.e., indirect indicators), and species and their distributions (i.e., more direct indicators). However, we must take care not to fall into the trap of "goal displacement" (Hilborn 1992a), where indirect but easily measured indicators are mistaken for the actual long-term objective. Because indicators are often imperfect surrogates for actual objectives, it may be wise to monitor a range of indicators.

Of course, for any monitoring scheme, the actual indicators monitored, the scale and precision of sampling, and other details will depend on the management objectives, as well as feasibility and cost. No broadly applicable cookbook of steps is available, although guidance is available for particular subject areas (e.g., hydrology and watershed integrity, biological diversity).

While it is relatively easy to blindly gather data, it is a much more difficult task to design monitoring programs that are relevant to management objectives, statistically credible, cost effective, and practical. This is particularly true when there are multiple management objectives, and when monitoring must be co-ordinated over a range of spatial and temporal scales. The Clayoquot Sound Scientific Panel (1995) points out that: "Nowhere has sufficient effort been invested in this critical aspect of ecosystem management" and argues that: "It is worthwhile to devote considerable effort to devising the simplest methods that will give informative results." It may be useful to quantify the trade-offs between the cost and informativeness of different indicators or monitoring schemes. It may be worthwhile to invest effort in developing novel monitoring techniques that are both inexpensive and effective. For example, J. Henshaw (USDA Forest Service, pers. comm., 1995) described the use of ultralight aircraft and video cameras that collected extensive (but coarse) data, relatively cheaply.

Monitoring programs must be designed to withstand staff turnover and possible interruptions in funding. Methodologies must be clearly articulated and documented. Because monitoring can potentially generate vast amounts of data, effective and efficient systems for analyzing, storing, and retrieving data are critical, in order to avoid significant logistical problems.

Who monitors?

Designing and co-ordinating effective monitoring programs will require the input of both managers and researchers. Managers are required to ensure that monitoring programs address key questions and do not get bogged down in the interesting research questions of secondary importance to making management decisions. The design process should involve researchers with backgrounds in natural and social sciences, and specialists in experimental design and statistical analyses.

Some data will need to be collected by those with specific technical expertise and skills. Much monitoring can be carried out by technical staff from government or industry. Monitoring activities that require frequent, relatively simple measurements are well-suited to participants from local communities who have access to the forest and are interested in working regularly and conscientiously (Clayoquot Sound Scientific Panel 1995). B.G. Marcot (pers. comm., 1995) noted that an effective means of motivating those involved in monitoring is to tie results of monitoring to pre-set decision points so that those monitoring can see the immediate utility of their efforts. Involving local residents in monitoring can have a number of benefits (e.g., cumulated experience and familiarity, local commitment to long-term continuation of the program, reduced labour costs), but it will also require training (with the associated investment of time and money) and an appropriate administrative structure (Clayoquot Sound Scientific Panel 1995).

5.6 Feedback loops

Role

"If you cannot respond to what your have learned, you really have not learned at all" (Hilborn 1992b).

Unless results are used to modify management activities or objectives, the time, effort, and money invested in experimental design and monitoring will be squandered. Specifying at the outset how information will be used to adjust management will facilitate its timely and appropriate application. It will also ensure that we are indeed answering questions relevant to management decisions.

How do we do it?

Predicted responses to alternative treatments and how those responses will affect future management activities should be documented when the management experiment is designed. These explicit "feedback loops" will provide a framework to encourage and guide change, even though actual responses may not be as clear as those predicted, and thus appropriate modifications may not be as simple as those initially specified. Defining at the outset how and when certain responses will change management actions or guidelines can alleviate "panic" responses to unfavourable preliminary results, and can make it easier to implement unpopular changes.

It will be necessary to establish some guidelines for what types of monitoring information should result in changes to management activities. For example, those involved need to decide how much of a negative result is needed to effect a change in management, and whether preliminary results are a sufficient basis for change. Preliminary data may be less reliable, but waiting for more complete information may allow irreversible detrimental changes to occur.

Predetermined changes (qualitative or quantitative) in key indicators should trigger predetermined changes in management activities or guidelines. These trigger points or "thresholds of acceptable change" should be defined for a variety of time frames, so that changes in management are not unnecessarily delayed by indicators with long response times. Preliminary data can serve as "early warning signals," triggering adjustments in management to avoid irreversible detrimental changes. The size of these adjustments should reflect some balance between the reliability of the data and the potential cost of not adjusting activities.

In addition to specifying how information will be used, it is important to identify *who* needs *what* information, *when*. Procedures should be developed that ensure the timely transfer of information to those who need it, and to those with the authority to effect change.

Workshops that involve participants with diverse skills, knowledge, and perspectives are a valuable tool for clarifying assumptions and exploring innovative solutions.

6 Tools for Implementing Adaptive Management

There are several tools that are potentially useful, although not essential, for designing and implementing adaptive management projects. AEAM workshops may be useful for exploring the potential effects of alternative policies and identifying key questions. Decision analysis can be a useful tool for evaluating alternative experimental designs and monitoring schemes, or assessing the risk of alternative strategies. Statistical power analysis, while not discussed here, can be used to compare the statistical value of alternative designs. Project design teams could assist in the overall design of adaptive management projects.

6.1 AEAM (Adaptive Environmental Assessment and Management) workshops

Role

AEAM workshops can have a range of objectives and can serve a range of functions (Holling 1978; ESSA 1982). They can be used to structure a resource management problem, identify key uncertainties, screen policy options, and explore "what if" scenarios about the impacts of various management activities. The AEAM workshops provide a forum for gaining input from a variety of people, thereby enhancing communication and stimulating creative problem-solving. The benefits of AEAM derive primarily from the *process* of building a model, rather than from the predictive capabilities of the model itself.

What is AEAM and how does it relate to adaptive management?

AEAM is defined by ESSA (1982) as "a collection of concepts, techniques, and procedures intended for the design of creative resource management and policy alternatives." Modelling workshops are the central and most well-known component of aeam; in many cases, they are the only component applied. AEAM and adaptive management overlap, and are sometimes mistakenly used interchangeably, but they are not synonymous. Adaptive management does not necessarily use the AEAM workshop methodology, and involves the *implementation*, *monitoring,* and *adjustment* of policies, in addition to their design.

"AEA . . . uses the construction of dynamic models as an intellectual device to help people clarify issues, communicate effectively about shared concerns, and explore objectively the consequences of alternative policy options" (*Walters 1986*).

What do AEAM *workshops involve?*

In AEAM workshops, participants usually develop a simulation model of a resource management problem, applying techniques of systems analysis (ESSA 1982; Walters 1986). The model is used to explore the potential impact of a variety of management alternatives on defined performance indicators, in an atmosphere of "game-playing" (Holling 1978; Walters 1986). The purpose of the workshops is not to develop a detailed, accurate, predictive model, but rather to use the process of developing and playing with the model to clarify the problem, uncover uncertainties, show the implications of some assumptions, and screen policies for further testing (Walters 1986; Lee 1993). The model is intended to explore "what if" scenarios, not to provide accurate predictions.

The AEAM process may involve one or a series of workshops, of varying lengths (Holling 1978; ESSA 1982). The first workshop is used to bound the problem, identify policy alternatives and performance indicators, and develop and run a preliminary model. In subsequent workshops, this model is refined, based on independent work done between workshops, and further explored. AEAM workshops were effectively used to explore strategies for rehabilitating salmon stocks in the Columbia River basin (Lee and Lawrence 1986; Orians 1986; Volkman and McConnaha 1993), and restoring hydrological patterns and wading bird populations in the Florida Everglades (Walters et al. 1992).

Who is involved?

The mix and quality of participants in the workshop are critical to its success. The workshops may involve 20–30 people, with a variety of expertise, knowledge, and talents, including:

- researchers from a variety of disciplines;
- managers;
- decision-makers;
- operations staff;
- modelling team/facilitators; and
- stakeholders/public (ESSA 1982; Walters 1986).

Participants should not only have the relevant professional expertise, they should also be creative, innovative thinkers. In particular, the review by ESSA (1982) emphasizes the key role played by a so-called "wise person." The wise person has both technical expertise and an understanding of the institutional environment, believes in the potential value of AEAM for helping with the problem at hand, and holds the respect and credibility of other participants.

Potential benefits

The potential benefits of AEAM summarized below may be realized to a greater or lesser extent in different cases, depending on the intent and success of the workshops. In addition to these intangible benefits, products of AEAM include: the simulation model, a report describing the model and summarizing workshop proceedings, and in some cases, presentation packages (ESSA 1982).

Identifies key uncertainties — The AEAM workshops can be an effective tool for identifying key issues and gaps in knowledge (ESSA 1982; Walters 1986; Volkman and McConnaha 1993). The systems approach of AEAM, together with the process of building a simulation model in a limited amount of time, can help to distinguish between significant and insignificant uncertainties. Resources can then be focused on resolving those uncertainties that affect management decisions, rather than being diffused over a large number of less significant uncertainties.

Demonstrates potential outcomes of management alternatives — The model can provide a consistent framework for comparing management alternatives, and can be used to screen out those that are obviously ineffective or counterproductive (Walters 1986; Lee 1993; Volkman and McConnaha 1993). By managing and integrating large amounts of information, the simulation model can also help participants see the implications of their assumptions. This can often stimulate new approaches to the problem, shifts in priorities, and the design of creative management alternatives (Walters 1986; Lee 1993).

Enhances communication — An AEAM workshop can enhance communication amongst people from a variety of disciplines and with a variety of roles (i.e., researchers, managers, stakeholders) (ESSA 1982). It can provide a forum for building common goals and a common understanding of the problem. In addition, the focus on problem-solving and comparing alternative scenarios can encourage participants to leave entrenched positions, and move beyond arguments over values (T. Webb, ESSA

Technologies Ltd., pers. comm., 1995). Enhanced communication can lead to improved co-operation and more effective management.

Stimulates creativity and generates new ideas — AEAM can create an atmosphere in which creative alternatives and approaches are more likely (although not guaranteed) to emerge (ESSA 1982; Walters 1986). Uncertainties are openly acknowledged, management alternatives are tested in non-threatening "gaming" with the model, the implications of assumptions are demonstrated, and participants are exposed to ideas from a variety of disciplines and perspectives. All stimulate new modes of thinking that can lead to creative solutions.

Encourages changes in attitudes — Although AEAM workshops cannot by themselves bring about changes in attitudes and institutions, they can act as "instruments of change" (ESSA 1982). The AEAM methodology explicitly recognizes risk, uncertainty, and the possibility of failure, and encourages participants to develop new (and potentially risky) management alternatives. Thus, AEAM workshops may help to break down some of the institutional barriers to adaptive management.

Potentially saves time and money — By identifying key uncertainties and issues, AEAM workshops can help to focus effort, time, and money where they will have the greatest benefit. An AEAM workshop could also help "kickstart" an adaptive management program, potentially reducing the amount of time and money wasted on unproductive activities.

Problems

AEAM workshops require hard work, energy, time, and money, yet there is no guarantee that these investments will pay off. The benefits are often difficult to quantify, may only be realized fully over the long term, and are dependent on the commitment of participants to apply and internalize workshop results (ESSA 1982). The success of the workshop itself is highly dependent on the quality and commitment of both the participants and the workshop facilitator/modeller.

Success of the workshop depends on the quality and attitude of participants — Participants must not only have the necessary professional expertise, they should also be creative thinkers, with a knack for coming up with new alternatives (ESSA 1982; Walters 1986). Identifying and involving the "right" people can be difficult. Today, some may be reluctant or unable to commit time and energy because of the concurrent and overwhelming demands of the FPC. Others may be "burned-out" from other processes (e.g., Commission on Resources and the Environment).

Success of the workshop depends on the skill and attitude of the workshop facilitators/modellers — There are relatively few people skilled in running AEAM workshops; securing a good, experienced facilitator/modeller may be difficult. They play a number of critical roles, including that of modeller, facilitator, information critic, and intellectual leader (ESSA 1982), that require diverse talents and skills. The facilitator/modeller must be able to articulate quantitative concepts, translate ideas into quantitative models, create an environment that stimulates innovation, and avoid destructive misunderstandings amongst participants. They must be willing to listen to participants, and admit and learn from mistakes. They must not allow technical, modelling problems to overwhelm broader workshop objectives.

Success depends on the commitment to apply and internalize the lessons from the workshop — AEAM workshops can produce some immediate benefits (e.g., identification of key uncertainties, enhanced communication, new management alternatives), but the full potential will not be realized unless ideas are implemented and the attitudes and methods are internalized (ESSA 1982). AEAM alone will not result in adaptive management, nor overcome institutional barriers.

The workshop may not produce a useful model — It is unlikely that a useful, reliable model can be developed within the time limits of a one-day workshop, yet multi-day workshops require a larger investment of time and money. The benefits of gaming, screening alternatives, and visualizing assumptions will not be realized if the model is obviously unreliable or wrong. Participants may become frustrated or bored by the technical problems and unreliable results. This can lead to frustration or a loss of interest in the entire AEAM process. Some participants may resent the perceived waste of time.

AEAM workshops are expensive — AEAM workshops are expensive, and that expense is incurred immediately (ESSA 1982). The initial expense may be difficult to justify since the benefits are difficult to quantify and are realized only over the long term. Moreover, the investment may not pay off, since the benefits are not guaranteed.

Summary

AEAM workshops are most useful for identifying key issues and gaps in knowledge, exploring policy alternatives, enhancing communication, and synthesizing information. Consequently, they are better suited for addressing broad policy questions than narrow, focused problems (ESSA 1982), and are more effectively used early on in a process, rather than after it is well under way. It is vital to include innovative thinkers, with a range of expertise and perspectives, but ensuring their participation may be difficult. In general, multi-day workshops are more effective and less frustrating (but more expensive) than single-day workshops (ESSA 1982). To minimize potential frustration and disappointment, workshop objectives and expectations must be clearly understood by all participants, including the modeller/facilitator. Technical, modelling issues must not be allowed to overwhelm conceptual issues.

6.2 Decision analysis

Role

In the context of adaptive management, decision analysis can be used:

- to weigh the costs and benefits of an experimental versus a non-experimental approach to a specific problem; and
- to weigh the costs and benefits of alternative experimental designs and monitoring regimes.

What does decision analysis involve?

Decision analysis refers to a variety of tools for assessing the costs and benefits of management alternatives, taking into account the probabilities of different outcomes. Formal quantitative decision analysis involves outlining the following:

- the various alternatives;
- the potential outcomes of each;
- the probability of each outcome; and
- the value (or cost) of each outcome (McAllister and Peterman 1992a, b)

The expected value of a given alternative is determined by multiplying the probability of each outcome with its value, and summing these for each possible outcome of the alternative. Values (or costs) that occur farther in the future are often "discounted" to account for the economic notion that benefits in the future have less value than benefits today. Decision analysis can also be done qualitatively, using the same framework of alternatives and outcomes, but expressing the probabilities and values of outcomes in qualitative terms (Maquire 1991).

Benefits

"Humans are notoriously poor at making choices when there are significant uncertainties, conflicting objectives and complex interactions . . . Even a moderate investment in analysis encourages the deliberate consideration of the full range of consequences and associated values, and enhances our ability to make consistent choices" (*Maquire 1991*).

Decision analysis has the following general benefits (McAllister and Peterman, 1992a):

- provides a framework for structuring comparisons;
- makes decisions less arbitrary and more "transparent";
- maximizes the probability of choosing the best alternative (e.g., monitoring regime, experimental design); and
- allows decision-makers to explicitly incorporate risk and uncertainty into the comparison of alternatives.

Decision analysis can be a powerful tool for demonstrating the economic value of a management experiment. For example, McAllister and Peterman (1992a) (see Appendix 1) used quantitative decision analysis to compare the expected value of an experimental and a non-experimental fishing strategy. Under most conditions simulated, the expected economic value of the experimental strategy was greater, in some cases by up to 60%.

Decision analysis can also be useful for comparing the economic value of alternative experimental designs. For example, Sainsbury et al. (1994) (see Appendix 1) used quantitative decision analysis to determine the optimum length of the experimental phase of a strategy for managing multi-species fish stocks. The analysis considered the revenue generated during the experimental phase, the value of information gained, and the probability of discriminating between alternative hypotheses. For experimental periods longer than 15 years, the cost of experimenting (in terms of foregone revenues and monitoring costs) exceeded the value of the marginal gain in information. Another example of the potential use of decision analysis in comparing alternative experimental designs is described in Keeley and Walters (1994) (see Appendix 1) with regards to the British Columbia Watershed Restoration Program.

Problems

- At present, few managers are familiar with the benefits or techniques of formal decision analysis.
- Assigning numbers to benefits, costs, and probabilities can be difficult.
- Quantitative decision analysis can be computationally intensive.

6.3 Project design teams

Teams of "experts" could be used to aid in the design of entire adaptive management projects or components of projects (e.g., experimental design, monitoring). Such teams would make optimal use of the high-quality, but scarce, expertise available in British Columbia (see Section 7.2), and would be a valuable resource for managers implementing adaptive management. Teams could be made up of experts from within a region, or could draw experts from throughout the province, depending on the skills that are required and available. Using teams to design projects would ensure good integration and compatibility among different elements of a project (e.g., experimental design, monitoring, statistical analysis).

7 Policy, Institutional, Social, and Organizational Issues

Institutional, social, and organizational components of management pose a number of challenges to the application of adaptive management. These challenges are grouped into nine main categories below. For each, specific issues are noted and some solutions suggested. Note that not all issues in each category have suggested solutions. The issues summarized in this section are derived from the experiences of other jurisdictions. In many areas of British Columbia, the history of co-operative, interagency research conducted by government researchers, university researchers, students, land managers, and industry co-operators has established a strong foundation for adaptive management. This foundation will need to be expanded and developed for adaptive management to succeed.

7.1 Professional managers are sometimes reluctant to admit uncertainty, and reluctant to risk the less than optimal outcomes that may result from innovative management alternatives

Issues

1. Traditional career paths do not support adaptive management. Career success is usually based on attainment of short-term, narrowly defined management objectives. It does not recognize the value of learning, nor the necessity of making "mistakes" in order to learn. Managers are often concerned with maintaining their position as recognized technical experts. This definition of success is incompatible with the focus on experimentation, innovation, and learning that characterizes adaptive management.

2. In the current management environment, managers may be reluctant to admit uncertainty regarding appropriate management practices because of concern that this might promote even greater debate and greater challenges to management mandates and agency authority. Some may be concerned that admitting uncertainty would allow some interest groups to further challenge particular practices and gain "political capital." Uncertainty needs to be recognized and accepted as inherent to ecosystem management; "complete knowledge" should be neither expected nor demanded by managers, decision-makers, resource users, or the public.

3. Perceived "failures" or less than optimal outcomes are an inevitable result of testing a range of management alternatives and a necessary by-product of rapid learning; nonetheless, they are a potential source of embarrassment to managers and may lead to severe criticism by the public, other agencies, or industry. It is vital to acknowledge the value of management activities that may appear to be failures, yet generate valuable knowledge that ultimately leads to more effective management. There is also a need to change the management response to public pressure from one that leads to an ultra-conservative, standardized, "by the book" approach to management, to one that instead leads to innovation, site-specific prescriptions, and learning.

Solutions

1. Redefine success and failure to recognize the value of information and encourage innovation.

   (i) Managers should be rewarded for instituting management activities that lead to learning. They should be encouraged to include goals and components of adaptive management projects in District business plans. To create career incentives for doing adaptive management, formal performance evaluations should assess achievement of adaptive management goals and explicitly recognize efforts to learn and gain information. Management performance would then be evaluated on more than short-term goals and results. Formally rewarding activities that lead to learning would encourage the creativity and innovation that are keys to successful adaptive management of resources. Linking budgets to a manager's willingness to implement adaptive management projects will powerfully reinforce the importance of adaptive management.

   (ii) For academics, promotion, recognition, and reward systems, such as tenure and awarding of research grants, should be changed to recognize collaborative work in management experiments, in addition to work that leads to "pure" or "basic" scientific publications.

2. Embrace uncertainty. This may not be as difficult as it would have been even a few years ago. Many managers already recognize uncertainty and will welcome the opportunity to admit it explicitly and to develop projects to address it. Several provincial initiatives (e.g., Land and Resource Management Plans, Local Resource Use Plans) already recognize the changing role of the B.C. Forest Service from that of the sole decision-maker presumed to have complete knowledge, to that of a participant in a more inclusive approach where all participants are aware of the limitations of the available information.

3. Build public trust. In order to test a range of management options, some of which will have outcomes that are less than "optimal," public trust in government managers and in industry will likely need to increase. The public must be convinced that adaptive management is a real change. Public involvement in adaptive management projects will help build trust and will promote better understanding of the benefits and expectations of adaptive management (i.e., that some "failures" are part of adaptive management). Managers, resource users, and the public should accept that saying "I don't know, and here is how we're finding out" is more useful than providing a potentially wrong, "standard policy" answer based on inadequate information.

4. Introduce adaptive management as a way of testing uncertainties in the FPC and improving it over time. It can be viewed as a way of making the job of implementing the FPC easier, a way of resolving contentious issues, and a way of moving beyond current "command and control" institutional frameworks.

5. Use pilot projects to demonstrate how management activities that are deliberately designed as experiments help planning and management, and lead to refined standards and guidelines. Demonstrating the rewards of adaptive management will help to overcome reluctance to make mistakes. Management experiments may provide the additional benefit to industry participants of enhancing their image as socially responsive, good corporate citizens.

7.2 There is a lack of skill, expertise, and time to learn adaptive management approaches

Issues

1. Providing expert assistance in the design of adaptive management projects is not straightforward. There are few technical people with the necessary skill and experience in designing management experiments or in applying the variety of statistical analyses that may be appropriate. Statisticians familiar with controlled settings and classical statistics may be unwilling to expand their viewpoints to support, learn, and then teach non-classical approaches. Furthermore, many statistical "experts" do not fully appreciate the constraints imposed by management settings. They may be reluctant to co-operate on projects operating at the lower levels of statistical certainty required for management, as compared to science, since it may reflect badly on them in a career context. Piquing the interest of statisticians and scientists to explore and adopt approaches outside their traditional disciplines and interests may be difficult.
2. Training managers to use the tools and techniques required for adaptive management (including experimental design and statistics) is important because, although researchers and managers will be collaborators, managers will be implementing most of the on-the-ground management experiments. Managers are far more numerous than researchers. At the very least, managers need to know how and where to request technical support. Unfortunately, managers who are currently facing a barrage of training regarding the FPC may have little time available for additional training.
3. Avoiding burn-out of people involved in training for, or implementing of, adaptive management will be a challenge, as "experts" are few.

Solutions

1. Implement professional development workshops in the new approaches and tools useful in adaptive management. These workshops could be included as part of skills training packages developed to implement the FPC, rather than as separate "add ons." A cookbook approach is incompatible with adaptive management, thus workshops should encourage a broad understanding of issues and approaches in order to stimulate creativity in developing hypotheses and options to test. They should also familiarize staff with tools such as decision analysis and risk assessment. More comprehensive training in the use of particular tools could be postponed until the first round of FPC training is complete.
2. It will probably be necessary to draw on experts from outside the B.C. Forest Service to assist in learning new approaches. Assistance from experts in British Columbia universities and from the U.S.A. will be valuable.

3. Consider developing a provincial co-ordination team to assist in the development, design, and co-ordination of adaptive management projects, on a request basis, throughout the province.

7.3 Adaptive management requires commitment to continuity of funding, monitoring, and involvement of key people over the time frames necessary to detect ecosystem responses to management activities

Issues

1. Some adaptive management projects require commitment longer than fiscal budgeting cycles, election cycles, or forest industry business cycles, and therefore may be vulnerable to interruptions in support. Institutional patience and stability must be sufficient to support long-term investment, of both money and staff, in some projects.
2. The pressure for immediate solutions to land and resource use conflicts promotes "quick fixes" rather than long-term learning. Adaptive management may be able to contribute in both the short and long term, but should not be compromised by a demand for quick fixes.
3. Long-term projects pose logistical problems for the collection, analysis, storage, and retrieval of data. Staff turnover, especially the high turnover in "remote" districts, makes cohesion of long-term projects a challenge.

The greatest barriers are often institutional and social. Some potential barriers include: fear of failure, reluctance to change, inequitable distribution of costs, and short political and funding time horizons.

Solutions

1. Consider incorporating projects into regional planning processes (e.g., Land and Resource Management Plans) that involve local communities and co-operators. This may generate local support for continued, long-term funding. A long-term plan for funding a project should be incorporated in both local plans and in higher-level government and industry programs.
2. In order to encourage continued support, design projects to provide some useful information that can be incorporated into ongoing management in the short and intermediate terms, in addition to the long term. As much as possible, design projects to weather potential interruptions in funding.
3. Write a formal plan for each adaptive management project. This plan should include a clear schedule for treatments, monitoring, analysis, and feedback, and clear assignment of responsibilities for each task to specific positions or individuals.
4. For all adaptive management projects, develop a plan for managing the storage and release of data. Co-operators need to agree on who has access to data, and who shares the costs of data storage. Data manipulation, analysis, storage, and retrieval must be well planned so that the large amounts of data that will be acquired do not quickly become unmanageable. Compile and store maps showing location of plots, transects, descriptions of field methods, and other aspects of the experiment, so that such information is readily accessible.
5. Develop a clear staffing and "succession" plan for all projects. Overlap employment windows of outgoing and incoming staff to allow outgoing staff to train incoming staff in project details. A team approach to projects will likely increase

the probability of a number of people knowing the details needed to keep a project effective. A central system for co-ordinating project information and storing data (either primary data or backup copies) would likely be useful in ensuring project continuity; however, central "control," whether perceived or real, could significantly dampen local enthusiasm and initiative.

7.4 To yield useful information, an adaptive management project must be rigorously implemented, as well as rigorously designed

Issues

1. In a rapidly changing world there will likely be increasingly rapid changes in values, and social and economic pressures over the life of a project. These could lead to the project being abandoned, or the design being compromised, wasting the resources already invested. Despite their limited reliability, preliminary results may, in some cases, lead to pressure to abandon "unsuccessful" treatments, or to widely apply "successful" treatments, thus jeopardizing the experiment and its ability to provide reliable information.
2. Effective, rigorous management experiments can be difficult and expensive to design and implement; there may be pressure to use less expensive approaches, even if they are also less reliable, and to compromise experimental design or monitoring.
3. There will almost certainly be pressure to do things that will confound some management experiments (e.g., salvaging windthrow or insect damage, restoring watersheds after natural landslides). Such interventions could invalidate some project designs, and put at risk the large investments in long-term projects.

Solutions

1. Issues related to the proper and sustained implementation of the adaptive management program are related to issues discussed in Sections 7.2 and 7.3. As long as project continuity and expertise are maintained, the reasons for the adaptive management project and design will be able to be sufficiently well articulated to make the benefits of rigorous implementation compelling.
2. Where possible, design projects to allow testing of treatments that arise after the experiment has begun. This "open design" encourages ongoing innovation, and recognizes the pace of change; projects remain relevant rather than being relegated to providing historical comparisons. Experiments that contribute to increased understanding of ecosystem function, rather than merely testing alternative treatments without going through the rigour of generating alternative hypotheses, will be valuable even if values, objectives, and treatments change.
3. Before conducting any salvage or restorative work in a project area, thoroughly evaluate the potential impact of such activities on the value of results.
4. Consider using quantitative techniques (e.g., decision analysis, power analysis) to compare the value of different designs and weigh their costs (see Section 6.2). These techniques can also be used to show the long-term cost of using cheaper but less reliable or less powerful designs. In some cases, cheaper

designs may be adequate; in other cases, they may be merely a waste of money and time.

5. Encourage effective, ongoing communication among all participants (researchers, managers, operations staff), including those responsible for actually implementing experiments on the ground. Good communication increases the likelihood that the experiment will be implemented as designed.

7.5 Adaptive management projects must consider the desire for fair and equitable treatment of tenure holders, other resource users, and communities; the costs and benefits of management experiments may not be borne equally

Issues

1. Management experiments may restrict the availability of timber in some areas, or otherwise impose costs and foregone revenues. In order to meet statistical requirements for control, replication, randomization, and other aspects of experimental design, costs or benefits may be concentrated in certain areas, on certain communities, tenure holders, or operators.
2. Some management experiments will undoubtedly include timber harvest options that do not maximize the timber yield of every hectare. In the current highly competitive climate, where fibre is scarce, industry may be reluctant to give up short-term access to timber in exchange for long-term information. Without industry support, the current tenure system and commitments may make it difficult to gain the access to the landbase necessary to do long-term management experiments. Commitments to other resource users or communities may impose similar obstacles.

Solutions

1. Costs of adaptive management could be viewed as simply costs of doing business. Where the costs and benefits are not borne equally by all participants, consider ways of making the experimental design more equitable, while still gaining reliable information. Quantitative techniques can be used to compare the informativeness and costs of different designs or to demonstrate the long-term value of management experiments (see Section 6.2).
2. Alternatively, consider sharing costs and benefits (i.e., information) of management experiments among participants. There are likely a number of alternative ways of offsetting or balancing the costs and benefits among co-operators. For some projects, costs and benefits will be borne by the same company and thus may be considered an investment.
3. Programs such as Forest Renewal British Columbia may be useful in offsetting some of the costs of adaptive management.
4. Use decision analysis, education, and demonstrations of management experiments to promote the view (amongst managers, industry, and the public) that information has a direct value to management and that it is worth investing in management experiments to gain information.
5. Demonstrate the value of adaptive management (e.g., through pilot projects). Conceptual arguments alone are probably inadequate; theory must be linked to practice through concrete examples.

7.6 Adaptive management requires regulatory flexibility within the Forest Practices Code and other regulations, to allow testing of a range of alternatives

Issues

1. Because current management systems focus on compliance, enforcement, and penalties for variation from a standard set of prescriptions (or objectives), it may be difficult to implement a range of management treatments. Yet active probing of the response of the ecosystem over a range of conditions leads to the most rapid rate of learning; it is treatments outside the "status quo" that most need testing. District managers of the B.C. Forest Service may bear the "costs" of non-compliance with current regulations and so be adverse to testing options outside the current standards.
2. Public resource managers are often reluctant to advocate rule changes because once regulatory changes are made, they make more work for already overworked staff, are difficult to reverse or modify, and may require new, expensive, monitoring or auditing systems. Likewise, industry is often averse to rule changes, particularly if those changes lead to increases in operational costs or reductions in timber supply.

Solutions

1. To effectively practice adaptive management, there needs to be flexibility in the standards of forest practices that are applied to the landbase. The FPC guidebooks provide flexibility, but managers may be reluctant to take advantage of the flexibility for fear of being in "non-compliance" with the FPC, or they may interpret the FPC as being inflexible. In some areas of management it may be necessary to develop a legal process to sanction adaptive management projects that are evaluating more "radical" forest practices. Safeguards must be developed to ensure that such flexibility is not used merely to circumvent parts of the Code; experimental rigour and the potential value of information gained by such an experiment must be demonstrated.

7.7 Adaptive management requires a management system and structure that involves all participants in a team approach

Issues

1. Teams are required for a successful adaptive management program due to the wide diversity of skills required at all stages of a project and the need for commitment by a wide range of participants. Adaptive management should include the whole variety of stakeholders during initial identification of key questions and planning of monitoring programs and feedback mechanisms. Teams that have worked effectively in the past have been groups of people with similar views and operating approaches. However, the teams required in the future will be more diverse, and will bring together people with very different views and approaches. This diversity can stimulate creative solutions, but greater skills will be required to deal with "cultural" and style differences amongst people from different agencies and disciplines (i.e., industry vs. environmentalists; managers vs. scientists; public sector vs. academic; scientists from different disciplines).

2. Even communication among scientists will be challenging. Basically, two types of sciences are needed: (i) the examination of parts of the ecosystem (more traditional disciplines), and (ii) the integration of parts to understand the whole (newer disciplines of systems dynamics, modelling, and systems theory). Examining individual parts or processes generally involves using experimental procedures, narrowing uncertainty, and devising tests to reject hypotheses (see Section 5). The integration of the parts uses the results of the former, but identifies gaps, invents alternatives, and evaluates the integrated consequences and cumulative effects of management actions for a whole system. Uncertainty is high, and analysis of uncertainty becomes a topic in and of itself. Scientists from each type of discipline therefore have very different perspectives on the world and cross-discipline respect and credibility may be a problem. Ensuring co-operation and understanding amongst scientists from different traditional disciplines may also be challenging.

3. Communication within and among projects, and between project team members and constituents will be critical. The greater efficiency of small teams must be balanced with the need to involve all stakeholders. The choice of team members from each interest group will be important.

4. Time and energy are required to build effective teams, lay the foundations for good team work, and develop explicit processes for decision-making and conflict resolution, yet this should not be allowed to delay unduly the implementation of management experiments.

5. Given the wide range of motivations and "agendas," it is important that adaptive management not be used or perceived simply as a delaying tactic or a tool for diffusing conflict.

Solutions

1. Choose team members based on their concern for the resources to be investigated, ability to work with others, willingness to listen to and respect other opinions, creativity, ability to think conceptually, and specific expertise or knowledge. Members should not simply "represent" the views of their interest group. Ideally, some team members would be committed, stubborn, persistent people, who know how to work within and around institutional frameworks.

2. Involve both managers and researchers. Researchers and managers are each other's clients; each have different senses of risk, time, purpose, and rewards (integration vs. reductionism); each can facilitate different aspects of adaptive management; and each can assist in developing appropriate questions and hypotheses that will aid in the future development of management options.

3. The adaptive management team should develop a protocol, acceptable to all members, for working together, reaching decisions, and resolving disputes. They also need the resources to implement the protocol effectively.

4. Carefully facilitate meetings to ensure that activities move forward at a reasonable pace.

7.8 The need for multi-agency participation in an adaptive management program is important; however, the issues of traditional mandates, roles, and approaches will need to be resolved

Issues

1. Management problems cross agency jurisdictions and require co-ordinated multi-agency approaches. Respecting and working with different agency "cultures" will be challenging. Institutional politics may pose a further barrier to adaptive management. The shifting balance of power and control inherent in "shared decision space" and a team approach may threaten some participants (if they think they may lose and others may win in the traditional balance of power in resource management). The traditional roles of agencies will need to change to accommodate the closer links between academic, regulatory, research, environmental, and industry communities that are essential for successful adaptive management. Adaptive management must not, however, be used simply to justify a position or force change on another agency.
2. Adaptive management requires integration of the culture of science and culture of management. Management experiments are "messier," with more confounding factors, than experiments typically undertaken by most scientists. Scientists must be willing to learn more things with less statistical certainty. Some scientists will be reluctant to retreat from the statistical certainty and rigour that traditionally defines "good science." "Both practitioners and researchers must recognize that experimentation is not just a study; it is not just a program evaluation; it is a major process of organizational change." (L. Sherman, quoted in Garner and Visher 1988)
3. Involving First Nations requires ensuring respect for all participants and resources (and understanding what respect means and how it is interpreted by First Nations peoples), recognizing the importance of spirituality, reconciling different concepts of time, and developing means of including traditional ecological knowledge with scientific knowledge.
4. Issues about the flow and control of information, its proprietary nature, and its use/misuse in political activities will have to be resolved.
5. The reward system (career path, promotion etc.) within the diverse agencies will likely require modification for full participation of key participants in an adaptive management program (see Section 7.1).
6. Commitment, support, and buy-in from Ministry executives, district managers, scientists, operational staff, industry personnel (at various levels), environmental non-government organizations, and the public is crucial for success (see Section 7.5).

Solutions

1. Government agencies in British Columbia are characterized by less confrontational and bureaucratic approaches than those of other jurisdictions. This improves the chances that adaptive management projects will be successful.
2. Make learning an explicit goal of management. Ensure that results are available to all jurisdictions. This could reduce issues of control or competition.

3. Engage political support and public involvement. Such support is crucial to changing management activities, plans, and policies. Management experiments yield information, but information alone does not drive changes in policy.

7.9 Adaptive management requires strong, explicit links between the results of management experiments, and the use of those results to modify regulations and future forest practices

Issues

1. There should be a clear and explicit feedback loop and threshold values agreed upon that will ensure that information gained from adaptive management is incorporated in current management.
2. Some stakeholders will have a vested interest in maintaining the status quo; others will have a vested interest in radical change.

Solutions

1. Agencies should agree to instituting explicit, simple feedback loops. From the outset, establish pre-set decision points that will trigger certain changes, based on information gained up to that time.
2. If pre-set decision points or responses to certain levels or types of information are not articulated at the beginning of an adaptive management project, it is vital to get, at the very least, a firm commitment from participants to change management activities based on responses of the system to adaptive management treatments.
3. Develop a schedule of analysis and feedback, with explicit timelines, as part of an adaptive management plan (see Section 7.3).

8 Further Investigations

This paper provides an overview of adaptive management, and some of the tools and methods it uses. It may be productive to further investigate some of these tools and how they can be applied to adaptive management of forest ecosystems in British Columbia.

1. Investigate the utility for adaptive management of Bayesian statistics, Combining Information (Draper et al. 1992), and other alternative forms of analysis. If possible, work through some examples in detail.
2. Investigate the design of large-scale, long-term experiments in experimental forests, or in other disciplines.
3. Investigate the design and methods of analysis for unreplicated and uncontrolled experiments in medical and social sciences research.
4. Further investigate the cost and potential value of an AEAM workshop for forest management by contacting ESSA and participants in other workshops (e.g., Ecotrust, Northwest Power Planning Council).
5. Further investigate risk assessment and different decision analysis tools (particularly the use of Bayesian prior probabilities), and their strengths, weaknesses, and applications.
6. Provide examples of decision analysis that illustrate the steps involved. Investigate the use of existing software packages (do they exist? what are their strengths and limitations?).
7. Consider who would use decision analysis and what skills they require to do it.
8. Investigate methods for identifying large-scale spatial replicates, or accounting (statistically) for variation between areas that are not ideal replicates.

9 Conclusions

Adaptive management is a valuable approach to managing systems that are characterized by high levels of uncertainty and complexity. A structured approach to learning from experience (both mistakes and successes) enhances the rate of improvement in management. To be effective, adaptive management must be defined consistently and clearly; at present, it is often misinterpreted to mean simply changing management in response to new information. Adaptive management involves a more rigorous approach to problem analysis, monitoring design, implementation, and evaluation than is typical of conventional approaches to management.

Pilot projects could provide powerful demonstrations of the value of adaptive management, as well as providing examples of how to do it. Well-chosen projects could provide some quick successes needed to gain wider support for adaptive management and overcome some of the potential institutional barriers. In designing and implementing adaptive management projects, careful thought should be given to strategies for avoiding or minimizing barriers.

Adaptive management is particularly timely in light of recent adoption of the Forest Practices Code. However, the FPC may also exacerbate some of the potential barriers to adaptive management. The question is: are these barriers substantial enough to warrant delay? Will delaying the implementation of adaptive management improve its chances of success? We cannot know for sure, but probably not — barriers will always exist, and there is probably no “ideal” time to implement adaptive management. Now is probably a better time than most, given the need to evaluate and improve the FPC, as well as its potential to catalyze change. Most importantly, however, the sooner we start managing adaptively, the sooner we will gain information that can help us manage more effectively and adapt to changing values.

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Participants in a workshop on adaptive management contributed many useful insights to this discussion paper. In addition, the following people were interviewed by telephone or in person:

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APPENDIX 1 Case Studies of Adaptive and Experimental Management

Despite increased references to adaptive management in the literature, and increased calls for its use, there are still relatively few cases where it has been applied effectively. Some attempts have fallen short because of the problems listed in Sections 4 and 7. Others are still in the early stages of being implemented, so it is too early to assess fully their value and problems. In this Appendix we summarize a number of case studies that illustrate particular elements of adaptive management, and its application in a variety of situations. They are not intended to precisely define adaptive management, but rather:

- to illustrate different aspects of adaptive management (e.g., decision analysis, experimental design, AEAM);
- to indicate the range of issues to which adaptive management has been, or could be applied (e.g., wildlife, fisheries, and forest management, population versus ecosystem-level issues); and
- to highlight points we can learn in applying adaptive management to forestry issues in British Columbia.

Together, the case studies also illustrate the variety of ways in which adaptive management has been interpreted. All but three (examples 9, 10, and 11) are from published articles in which the authors explicitly refer to them as adaptive or experimental management. Whether one thinks these case studies are good or bad examples depends on how one interprets adaptive management, and the emphasis one places on different elements. Despite their inevitable shortcomings, we can learn something from each case study. After all, we can often learn as much from mistakes as from obvious successes.

Of the case studies summarized here, that of the Australian multi-species fishery (Sainsbury et al. 1994) perhaps best coincides with the definition we present in Section 1. Specific alternative hypotheses about the effect of management actions on fish population dynamics were tested in a management experiment. This allowed the identification of the best long-term management policy, from amongst several proposed. The experiment and monitoring scheme were thoughtfully designed (although there were problems with their implementation), with different experimental designs being evaluated quantitatively. The results of the management experiment led to changes in management.

The case studies included here are described — not analyzed or evaluated — based on information in published articles, in some cases supplemented by an interview with the author. We include a background summary of the issue, the key management questions, the details of the experimental design (if provided), the actual or anticipated problems, and actual or anticipated benefits, as perceived by the author(s). We also include any other lessons that may be valuable in applying adaptive management in British Columbia.

1. Missouri Ozark Forest Ecosystem Project (MOFEP)

Kurzejeski et al. (1993); E. Kurzejeski (Missouri Department of Conservation, pers. comm., 1995).

Background

In 1990, the Forestry and Wildlife Research staff in the Missouri Department of Conservation initiated a management experiment on state-owned forests in the Missouri Ozarks. The Missouri Ozark Forest Ecosystem Project (MOFEP) is designed to assess the effects of three silvicultural treatments (even-aged, uneven-aged, and unharvested) on a variety of biotic and abiotic indicators of ecosystem function. At the scale of each treatment unit (300–450 ha), MOFEP is monitoring: 1) composition and spatial distribution of woody vegetation, 2) diversity of herbaceous plant species, 3) diversity and productivity of forest birds, and 4) production of oak mast. A number of associated studies are also being done at the stand scale (e.g., density and diversity of small mammals, site productivity, forest litter invertebrates). To date, 3–5 years of baseline monitoring have been completed. Harvesting (first entry) will occur in the fall of 1995.

Key question

- What are the effects of different silvicultural systems on forest community and on ecosystem function?

Experimental design

- randomized block design;
- pre- and post-treatment measurements;
- three spatial replicates (300–450 ha) of three treatments;
- no temporal replication: treatments applied to all blocks in same year.

Identified problems

- Previous harvesting limited the number of spatial replicates and precluded temporal replication. Out of 32 000 ha of state forest, only 10 “compartments” met the specified pre-treatment conditions. The limited number of replicates (three) resulted in relatively low statistical power, and increased vulnerability to catastrophic disturbances (e.g., fire).
- Cost and logistics of monitoring also imposed a constraint on the number of replicates (precluding the use of replicates in federal forests).
- Implementation of harvesting plans in accordance with the experimental design was flagged as a potential problem.

Identified benefits

The treatments have not yet been implemented, but a number of potential secondary benefits have been identified, including:

- enhanced public understanding of forest management issues (through workshops and field trips);
- enhanced communication between resource managers and resource users;
- enhanced interdisciplinary co-operation, which has led to further collaborative research;
- improved inventories (e.g., identified additional locations of rare and

endangered species, developed estimates of proportion of overstory trees containing dens and cavities).

Factors that contributed to successful implementation

- administrative support and clearly mandated direction — interagency strategic plans identified the need for interdisciplinary research, ecosystem-level studies, and expanded research;
- adequate budget;
- from the initial stages, managers and researchers worked as partners in the design and application of the project; amongst other aspects of project, managers were involved in identifying research priorities and developing stand-level prescriptions; managers viewed themselves as equally responsible for the project;
- good communication, both formal and informal, among all parties involved (e.g., a meeting to review the project and provide updates is held every year);
- atmosphere that stimulated creativity and innovation: individual biologists were responsible for research direction within their programs and were given latitude to think and be creative.

Other things that we can learn from this example

- encouraged "partnerships" between the Missouri Department of Conservation and other research and management agencies (e.g., USDA Forest Service, universities) to broaden scope of study, distribute costs, and enhance outputs;
- developed a summer internship program with universities that uses students to collect data while providing them with field experience; this provided the large numbers needed to collect data, while helping to reduce costs;
- consolidating separate projects into single, integrated project was seen as a way of increasing the efficiency and value of research;
- initial reluctance by administrators was overcome by showing the possibility that the project could save money in the long term by improving management (although no formal, quantitative decision analysis was used).

2. Alberta Pacific Forest Industries (AlPac) study on forest fragmentation

Schmiegelow and Hannon (1993); F. Schmiegelow (University of British Columbia, pers. comm., 1995).

Background

A management experiment has recently been initiated to investigate the effects of forest fragmentation on birds in the boreal mixed forest in north-central Alberta. Proposed broad-scale harvesting according to existing operating rules may result in forest fragmentation. This fragmentation will undermine the commitment by AlPac to manage the forest sustainably and strive to maintain viable populations of resident wildlife species. Researchers modified an existing clearcut harvesting plan, to create two experiments. The first created fragments of old mixedwood forest of 1, 10, 40, and 100 ha; the second created fragments connected to riparian buffer strips. Both studies are examining effects on community, metapopulation, and population dynamics of forest birds. Pre-treatment conditions were measured in 1993, harvesting treatments were applied in the winter of 1993–1994, and preliminary responses were monitored in 1994. The study was initiated by researchers at the University of British Columbia and at the University of Alberta, and conducted on land licensed to AlPac, with their co-operation. Cutting plans were approved by staff in the provincial Forest Service. The study area was in part selected for its similarity to one that is the subject of a detailed, ongoing biodiversity study.

Key questions

- What are the effects of forest harvesting and resulting fragmentation on community and population dynamics of forest birds?
- What is the value of travel corridors for fragmented populations?
- What is the value of riparian buffer strips as habitat?

Experimental design

1. Fragmentation study

- three replicates of four treatments (1, 10, 40, 100 ha fragments of old mixedwood forest);
- fragments of consistent rectangular shape, isolated from adjacent forest by clear-cut of > 200 m on all four sides;
- three replicates of four controls (same size classes), in adjacent, continuous forest area of > 3500 ha, which will remain unharvested.

2. Corridor study

- three replicates of three treatments (1, 10, 40 ha) connected to 100 m wide buffer strip on one side;
- unconnected replicates and unharvested areas described above as controls;
- no temporal replication;
- sampling methodology is described in Schmiegelow and Hannon (1993).

Identified problems

- Researchers put extensive effort into developing a design that was agreeable to AlPac and the Alberta Forest Service, while still being rigorous and powerful enough to detect effects. All sides had to be flexible. The eventual design was the product of six design iterations.
- Past harvesting and current harvesting schedules constrained the number of suitable study areas, thus contributing to the problems in developing an acceptable design. Extensive groundtruthing was necessary to identify suitable replicates.
- In harvesting the first block, some instructions were misinterpreted. Subsequent blocks then had to be harvested in the same way as the first, rather than as originally prescribed.
- Ensuring that results are applied in the long term is a concern.
- Providing a repository for information collected in the long term is a concern.

- Management of a particular area is not the responsibility of a single licensee: AlPac has rights only to harvest deciduous trees; other companies have rights to harvest conifers.

Identified benefits

- enhanced communication and co-operation among AlPac, researchers, and Forest Service;
- increased understanding of the value of management experiments: AlPac is now initiating its own management experiments and is soliciting support from outside agencies;
- broader understanding of and support for adaptive management as a result of diversified funding base (the study is supported in part by contributions from several environmental organizations);
- preliminary results suggest that two-pass harvesting systems will not retain the diversity of bird species, and are being used to modify other harvesting plans within AlPac's license area.

Factors contributing to success

- persistence of researchers in searching for acceptable design;
- good, ongoing communication among researchers, Forest Service, and AlPac;
- support of Forest Service and AlPac. This support probably stemmed from recognition of the inadequate state of current knowledge combined with a written commitment by AlPac to manage sustainably, and a requirement (written into their license) to strive to maintain viable populations of resident wildlife.

Other things we can learn from this example

- Power analysis is a powerful tool for convincing people of the need for more controls and replicates than they would otherwise agree to.
- To ensure proper implementation of management prescriptions, it is important to talk directly to the contractors. Information does not necessarily filter down to all who need it.

3. Adaptive management of antlerless elk populations in Idaho

Gratson et al. (1993).

Background

In 1992, the Idaho Department of Fish and Game initiated a program for managing antlerless elk populations that included: (i) development of a model of elk population dynamics, and (ii) a management experiment to test alternative management strategies and underlying assumptions about ecological and socioeconomic processes. The objective is to determine harvesting rates for antlerless elk that would meet multiple management goals. Three different harvest rates were tested in a total of 11 Game Management Units, which ranged in size from 100 to 3000 km^2. These harvest rates will be maintained for 5 years (1992–1996). The size and composition of the elk herd was estimated before implementing the treatments, and will continue to be estimated in alternate years. They are also surveying hunter harvest, success, and effort.

Key questions

- What is the effect of hunting (harvesting) on population dynamics?
- Three alternative hypotheses were developed: (i) completely compensatory mortality, (ii) completely additive mortality, and (iii) "threshold" (below the threshold, harvesting is compensatory; above the threshold, it is additive)

Experimental design

- three control units (lowest harvest rate), four replicates of two experimental treatments (low and high harvest rates);
- geographic clusters of three units, one for each treatment; treatments were not randomly assigned;
- size and composition of elk population in each unit was estimated before treatment, and will be estimated by helicopter surveys in alternate years after treatment;
- harvest rates, success, and effort will be surveyed by telephone.

Identified problems

- wildlife managers were reluctant to allow complete randomization in assigning treatments to Game Management Units. Obtaining randomization required intense negotiations; in some cases, treatments were not randomized.
- difficulty obtaining target harvest rates. Harvest rates are affected by the number of permits that sell and by hunter success. Researchers may need to alter planned harvest rates to reflect those actually obtained in the first year.
- cost of monitoring: experiment requires 25–46% more survey time than normal. Helicopter surveys in some units are at risk because of budget constraints.

Identified benefits (potential)

- The increased understanding of population dynamics and harvest dynamics resulting from deliberately probing the system will be used to improve estimates of model parameters.
- The improved understanding, together with the predictive simulation model, will help managers to set harvesting levels more systematically. Management will be less dependent on the local experience and expertise in each Game Management Unit.
- Better understanding and better predictive models may allow harvest rates, which in the past were conservative, to be increased.

Other things we can learn from this example

- Managers may not immediately appreciate the need for randomization or other aspects of experimental design. It would be useful to demonstrate the enhanced reliability and value to managers of results from well-designed experiments.

- Researchers should carefully consider the level of detail and intensity of monitoring that is needed. Effective adaptive management experiments may not require the same detail and intensity in monitoring that is common in intensive basic research. Unrealistic monitoring demands may compromise experimental design or sampling efforts.

4. Adaptive management of nest box programs for wood ducks

Semel and Sherman (1993).

Background

Guidelines for managing local populations of nesting wood ducks were tested in a manipulative management experiments in two protected areas, one in northeastern Illinois and the other in central New York. Specifically, the experiments were designed to test two alternative hypotheses about reasons for extreme brood parasitism in box-nesting populations of wood ducks. Better understanding of the underlying biological reason for brood parasitism led to broadly applicable guidelines to reduce the problem. Researchers compared nesting efficiency and productivity under two management regimes. In one, nest boxes were placed in highly visible locations at high densities; in the other, nest boxes were dispersed and hidden in deciduous woodlands, to mimic the distribution and density of natural nest cavities.

Key question

- How and why does nest box placement affect brood parasitism? Two alternative hypotheses were tested: (i) excessive brood parasitism may result from low availability of nesting sites, and (ii) excessive brood parasitism is primarily due to the placement of nest boxes. Each hypothesis suggests a different management strategy.

Experimental design

- two treatments were implemented in adjacent areas at each of two locations (Illinois and New York);
- in one treatment, nest boxes were placed in visible locations over open water, 30–50 m apart; in the other treatment, nest boxes were hidden in deciduous woodland, 150–180 m apart;
- parasitism rates, number of parasitic eggs, and egg hatchability were measured.

Identified problems

- Results were confounded by lack of temporal replication and short time scale. Differences in parasitism rates noted in the first year may have been due to the addition of the hidden nest boxes to visible nest boxes already in place for several years.

Identified benefits

- Simultaneous testing of alternative hypotheses refuted previous assumptions about reasons for brood parasitism and suggested a more effective and efficient management strategy:

"... efforts to increase productivity are better spent on minimizing parasitism by positioning existing boxes in less visible locations than by increasing the number of nest boxes available."
"... over time, hidden nest boxes will allow populations to more nearly achieve their full reproductive potential than visible boxes."

- Specifically, by hiding nest boxes in woodlands, fewer are needed, maintenance costs are lower, and year-round access for maintenance is improved.
- Hiding nest boxes also reduces competition with starlings.
- Results allowed managers to use "broadly applicable guidelines," rather than relying on annual, site-specific descriptive data.

5. Proposed management experiment for size-selective fishing of pink salmon stocks in British Columbia

McAllister and Peterman (1992b); McAllister et al. (1992).

Background

The mean adult body weight in stocks of pink salmon off the British Columbia coast has significantly declined since 1950, causing significant declines in the economic value of the catch. Researchers assessed several possible experimental designs for rigorously testing alternative hypotheses about the cause of the decline. They also evaluated the potential economic performance of the experimental and current non-experimental management strategies. A model was used to simulate the experimental approach, which combined size-selective fishing and non-selective fishing, and the non-experimental approach, which used only non-selective fishing. The simulations involved four fishery areas adjacent to the mouth of separate spawning streams. In the simulations, the experimental phase ran for 10 years, and performance was evaluated over a total of 20 years (i.e., 10 years after the end of the experimental phase). The model predictions were not tested in an actual management experiment.

Key question

- What is the underlying biological mechanism responsible for declines in adult body weight? A number of alternative hypotheses have been suggested: (i) decline is due to selective removal of large fish by fishing gear, combined with heritability of growth rate, (ii) decline is due to changes in oceanographic conditions, (iii) decline is due to intra- or interspecific competition, and (iv) decline is due to interstock selection (stocks with large fish are selectively depleted). This proposed experiment is designed to test the first hypothesis.

Experimental design

A number of designs were compared for their statistical performance. The best of these (summarized below) was then used to compare the economic performance of the experimental and non-experimental strategies.

- block design;
- two replicates of two treatments (size-selective and non-selective fishing gear);
- in each of the four spatial replicates, treatments were alternated annually to

control for area-specific effects; this design incorporates eight distinct salmon populations (because of the biennial life cycle);

- terminal fisheries (i.e., at mouth of stream) were simulated to reduce interception of fish from other stocks, thus ensuring intrastock (not interstock) selection;
- trends in mean annual weight would be monitored.

Identified problems (potential)

- willingness of sufficient numbers of fishermen to participate;
- lack of confidence by managers in the two key hypotheses, leading to lack of interest in an experimental strategy;
- finding three suitable spatial replicates;
- developing appropriate gear regulations.

Identified benefits (potential)

- Alternative hypotheses noted above suggest different optimal management strategies. By discriminating between these hypotheses, managers can impose the most effective alternative. "With an experimental strategy, managers could expect a greater chance of identifying the best long-term policy than with any non-experimental alternative." (McAllister et al.1992)
- The expected economic value of the experimental strategy exceeded that of the non-experimental strategy under most conditions, in some cases by up to 60%.
- The costs of the experiment would be confined to a small area and a few stocks, but the benefits would be widely applicable.

Other things we can learn from this example

- illustrates how quantitative decision analysis can be used to evaluate alternative management strategies;
- illustrates how the statistical performance of alternative experimental designs can be compared objectively, based on analysis of statistical power. This information can then help in deciding between different designs (practicality, acceptability, and economics would be other considerations in deciding between designs). For example, McAllister et al. (1992) found that for this example, a simple block design was at least as effective as a more complex "staircase" design.

6. Experimental management of an Australian multi-species fishery

Sainsbury (1987); Sainsbury et al. (1994).

Background

In 1985, a management experiment was implemented for a multi-species fishery on the North West Shelf of Australia. Commercially undesirable changes in species composition were observed in the area following the introduction of a commercial trawl and trap fisheries. There were several alternative ecological explanations for these changes, each of which suggested different management alternatives. A management experiment was implemented to discriminate between these alternative hypotheses. The information gained from this experiment would lead to more effective management and higher economic returns. Quantitative decision analysis was used to compare the potential economic performance of experimental and non-experimental strategies, and to refine the experimental design. The experiment included three management zones, each of which covered the continental shelf adjacent to about 80 nautical miles of coastline. The first 5 years of the experiment have been run; the experimental approach will continue (for an unspecified period) with some modifications to the experimental design and survey frequency.

Key question

- What is the biological mechanism underlying changes in fish species composition? There are four suggested alternatives: (i) intra-specific competition, (ii) and iii) two different inter-specific mechanisms, of which one is influenced by harvesting, and (iv) trawl-induced changes in benthic habitat that affect different species differently.

Experimental design

- three adjacent management zones: two closed to trawl fishing for 5 years, one starting in 1985, the other in 1987; trawling was maintained in the third zone throughout the experiment;
- trap fishing was permitted throughout;
- annual surveys were conducted for 5 years to monitor fish abundance and benthic habitat.

Identified problems

There were some difficulties in implementing the experiment. Specifically:

- reduced temporal contrast as a result of unexpected decrease in trawling in the second zone following closure of the first zone (i.e., between 1985 and 1987);
- trawling occurred in second zone (following planned closure) as a result of unexpected changes in management jurisdiction and the unexpected development of a domestic trawl fishery (after 1990);
- interrupted monitoring as a result of changes in the research organization.

Identified benefits

- Experiment provided good hypothesis discrimination, and indicated support for hypothesis that changes are a result of habitat modification.
- Results suggest that recovery of benthic habitat after trawling (and therefore fisheries recovery) is slower than previously assumed.
- These findings suggest difficulties in maintaining a viable trawl fishery, and suggest the development of fisheries that do not remove benthic organisms.

In addition, the management experiment has:

- improved estimates of model parameters;
- increased attention to habitat modification in evaluating management alternatives;

- improved information on dynamics of fish populations and benthic habitat;
- improved information on economic viability of domestic fisheries; and
- encouraged further research on habitat effects.

Factors contributing to success

- well-designed experiment;
- clearly articulated hypotheses.

Other things we can learn from this example

- Illustrates the application of a Bayesian approach where prior probabilities are placed on alternative hypotheses. The net present value expected from various experimental designs was estimated by calculating net present value conditional on each alternative hypothesis being true, and multiplying this by prior probabilities initially placed on each hypothesis. This information was then used to set the optimum length of the experimental phase, and to identify the optimum experimental regime (e.g., an experimental period of less than 5 years was not adequate to discriminate between hypotheses, while periods longer than 15 years resulted in experimental costs, in terms of foregone revenues, that were greater than the value of improved information).
- Experimental management can sometimes overcome problems resulting from changes in organizations and support, and contamination of the experimental design.

7. Water management in the Everglades

Walters et al. (1992).

Background

Hydrologic regimes in the Florida Everglades have been drastically altered as a result of urban and agricultural development. These changes have resulted in drastic declines in nesting populations of wading birds (amongst other impacts). Past efforts at restoration have focused on re-establishing hydrologic patterns in freshwater marsh areas currently used by wading birds. They have been relatively ineffective. A series of AEAM modelling workshops was held to develop a quantitative model of ecological responses to changes in hydrologic regimes. The workshops highlighted a number of key uncertainties that could only be resolved through management experiments. They also clarified a number of alternative hypotheses to explain the decline, which suggest different restoration strategies. Walters et al. (1992) propose an experimental regime of increased water releases to estuaries, to test one of these alternative hypotheses: that estuarine restoration is necessary for recovery of wading bird populations. This approach is more aggressive than past efforts at restoration.

Key question

- What is the cause of declines in nesting populations of wading birds? There are four alternative hypotheses: (i) distant magnets: nothing wrong with Everglades habitat, but birds attracted to better habitat elsewhere, (ii) loss of transitional habitat (used between rainy and dry periods), (iii) alteration in hydropattern, and (iv) estuarine degradation.

Experimental design

- Not described.

Identified problems (potential)

- Implementing some of the proposed experimental policies will have substantial impacts on other ecological and social components of the system and will be expensive (e.g., restoring larger, more natural flows will require diverting water from other uses, and building levees to protect residential areas; proposed concentration of flows to the lower Everglades will reduce seasonal flooding upstream that helps to control invasion of exotic species).
- Some policies potentially could cause irreversible damage, could be counterproductive, and may foreclose other options and should therefore be implemented cautiously.
- Control and replication are difficult.

Identified benefits (potential)

- Experimental management may reduce costs of managing the system by focusing efforts on a few key elements, and by reducing interagency fighting over specific, individual plans.

Other things we can learn from this example

- illustrates importance of considering (and testing) several alternative explanations for an observation or problem;
- illustrates importance of considering entire system, not merely subcomponents of system: past efforts focused on relatively minor interventions in protected areas only, and have been relatively unsuccessful. Walters et al. (1992) consider the entire Florida Everglades system, and suggest more aggressive and experimental interventions.
- illustrates possible application of adaptive management to a large-scale, complex problem in an area where experimentation carries substantial risks and costs, and where extensive modification of natural hydrologic regimes and ecosystems means that treatments may have unexpected outcomes (i.e., imposing natural flow regimes may not restore natural habitat and conditions because of large changes to the system);
- illustrates application of adaptive management in a system where replication and control are difficult;
- the proposed experimental restoration program includes a range of policies from “quick and dirty” tests in a relatively small part of the Everglades, to long-term and more cautious tests;
- focuses on concepts of: (i) trying a range of alternatives, (ii) systems analysis, and (iii) experimentation. This contrasts with focus of other examples (e.g., AlPac, pink salmon) on relatively narrow problems, and on issues of experimental design.

8. Columbia River Basin / Northwest Power Planning Council

Lee and Lawrence (1986); Orians (1986); Lee (1989); Volkman and McConnaha (1993).

Overview

The Columbia River Basin Fish and Wildlife Program, run by the Northwest Power Planning Council, has the mandate to restore fish and wildlife to the basin, to compensate for damage caused by hydro-power development. The program primarily focuses on the recovery of salmon stocks. A policy of "adaptive management" was formally adopted by the Council in 1984, at the suggestion of Council member, Kai Lee. Lee saw adaptive management as a way of dealing with the high levels of biological uncertainty and of providing a strong conceptual basis for the implementation of the rehabilitation program. Volkman and McConnaha (1993) write that "the Council would be the place where key hypotheses would be identified and experimental designs considered, but also a political forum where all interested parties would participate in the debate."

The power and mandate of the Council are legislated in the Northwest Power Act. The Act's emphasis on prompt action and a system-wide approach are consistent with adaptive management. A range of groups participate in the recovery program, which is funded by the Bonneville Power Administration from hydro-power revenues. The management recommendations are implemented by a variety of organizations and agencies.

Starting in 1986, a series of AEAM workshops was held to develop and refine a system-wide model of salmon dynamics in the Columbia River Basin, and to establish clear objectives for salmon recovery. The model has been used in systems planning exercises, conducted by the fish and wildlife agencies and Indian tribes, to explore alternative recovery strategies. The program also identified areas of emphasis where research should be focused (e.g., increasing hatchery production, improving downstream transport of juveniles). This helped to organize management opportunities and issues into four categories (e.g., habitat manipulation, hatchery development). The program also identified four tributaries that would provide good opportunities for management experiments. Different issues would be addressed in the different tributaries (i.e., they are not replicates). For example, the Yakima Fishery Production Project is testing an alternative hatchery strategy aimed at improving stocks of wild salmon.

Management experiments have not always been implemented effectively. For example, the benefits of transporting juvenile fish by truck are open to question because the experiment failed to provide a control group (non-transported) in years of low flow. Managers assumed (perhaps incorrectly) that transportation was beneficial, and felt that the risk to weak stocks of not transporting in low flow years was unacceptably high.

The formal commitment to adaptive management has in general led to more rigorous thinking and problem analysis, but it is unclear to what extent manipulative management experiments have been used. The rehabilitation program faced significant barriers.

Identified problems

- The power of the Council is constrained; it can guide, but not command, river management. The ultimate authority rests with the funding agency, the large and powerful Bonneville Power Administration (the federal agency responsible for developing and marketing hydro-power). In addition, the Council itself does not implement any projects; this is the responsibility of a variety of organizations (e.g., fish and wildlife agencies, Indian tribes, Bureau of Reclamation).

- Institutional complexity was an issue. A large number of organizations and agencies are involved (11 state and federal agencies, 13 Indian tribes, 8 utilities, and numerous other affected stakeholder groups), dispersing authority and making co-ordination difficult. “Probing biological uncertainties requires the co-operation of fish managers, who control access to test fish; Bonneville, which funds restoration projects; and the Corps of Engineers and the Bureau of Reclamation, which control the dams. This diffusion of authority increases the likelihood that one party will effectively ‘veto’ the action.” (Volkman and McConnaha 1993)
- Establishment of co-ordinated basin-wide monitoring, research, and information systems has been difficult. The CRB crosses numerous jurisdictions, and costs are an impediment (despite funding by Bonneville Power).
- Many of the issues in managing the Columbia River Basin deal with trade-offs between competing values and goals that ultimately cannot be resolved by adaptive management.
- Institutional inertia, fear of failure, aversion to risk, and perceived costs of undesirable outcomes have all been obstacles. These are discussed in general terms by Lee (1993).
- The Columbia River Basin is intensively developed. Consequently, some measures imposed a high cost on other groups. In particular, flow manipulations affected (and were resisted by) farmers, ratepayers, and utilities.
- There was no precedent for a rehabilitation project on such a large scale.
- Values and societal objectives changed during the course of the program. The program initially focused on rebuilding salmon runs for harvesting purposes, but in the late 1980s and early 1990s concerns about conservation of wild stocks were raised. Past Council initiatives were heavily criticized for their focus on hatchery-raised stocks.
- People and organizations are impatient for answers.
- The Act required the Council to rebuild salmon stocks, without jeopardizing an “adequate, efficient, economical, and reliable power supply.”
- There are significant biological risks to experimenting with a declining resource.

Identified benefits

- Adaptive management provided a strong conceptual framework for dealing with large, significant biological uncertainties.
- The system model enabled participants to: explore “what if” questions and alternative policies, identify gaps in knowledge, focus experimentation where it could be of most value, identify “promising” policies for further testing, and co-ordinate rehabilitation efforts.

There are undoubtedly numerous other more specific benefits to management; some are implied in the references cited.

9. British Columbia Watershed Restoration Program

Keeley and Walters (1994).

Background

The British Columbia Watershed Restoration Program is intended to accelerate the restoration of watersheds affected by logging, and particularly focuses effort on local, heavily disturbed sites within logged watersheds. The program will be implemented in co-operation with local communities and local stakeholder or stewardship groups. A planning workshop held in March 1994 addressed the following four issues:

1. What are the most powerful and efficient experimental designs to permit evaluation of the program and various restoration techniques?
2. What are the most appropriate response variables to monitor within the program?
3. What are the most appropriate restoration techniques?
4. How can technical innovation and community stewardship be captured and incorporated into the program?

Experimental design

The experimental design subgroup identified five key considerations:

1. Results may be confounded in retrospective experiments, where treatments within a stream reach that is already disturbed are compared.
2. The temporal scale required to observe responses is substantial.
3. The spatial scale must be large enough that local variations are unimportant.
4. Controls and treatments must be paired based on above considerations and appropriate indicators.
5. Fish are the primary biological indicators (for this program); other indicators may also be monitored.

The subgroup recommended a "triplet" design, involving the comparison of one control and two treatment watersheds. One of the treatments would involve major restoration; the other would involve partial restoration. Walters demonstrated how a simple decision model, run on a spreadsheet, could be used to evaluate alternative experimental designs (specifically, the optimal number of paired watersheds and the optimal duration of the experiment). The model calculated the expected value of a design, as the product of the "prior probability" of different responses, and the net benefit of each response. Contour plots showed systematically how the expected value varied with different design parameters (e.g., time, number of watersheds, capital and operating costs, monitoring costs). The model indicated that 8–16 pairs of watersheds, observed for 4–8 years would provide the most powerful comparison, with the best ratio of cost to benefit.

MANAGEMENT EXPERIMENTS IN THE B.C. FOREST SERVICE

Several projects aimed at testing the effects of alternative silvicultural systems on a number of ecological and economic indicators have been initiated in British Columbia. These include:

- West Arm Demonstration Forest
- Roberts Creek Silvicultural Systems Trial
- Montane Alternative Silvicultural Systems Project (MASS)
- Quesnel Highlands ESSF Trial
- Sicamous Creek Silvicultural Systems Project
- Date Creek Silvicultural Systems Project

Most of these projects include elements of adaptive management; however, adaptive management should be expanded beyond these discrete areas, and should address a broader range of issues than are addressed in most of these projects. Two of these projects (Sicamous Creek and Date Creek) are described below.

10. Sicamous Creek Silvicultural Systems Project

Murray and Bernard (1995).

Background

The Sicamous Creek Silvicultural Systems Project was not initially planned as an integrated "adaptive management" experiment, but nonetheless it does include elements of adaptive management (e.g., replicated treatments and controls in a management setting). The project was initiated in 1991 by the B.C. Forest Service, with the co-operation of the licensee, Riverside Forest Products. The study site is located in the Engelmann Spruce–Subalpine Fir (ESSF) wc2 biogeoclimatic subzone, in the Sicamous Creek watershed in the Kamloops Forest Region. Four different silvicultural treatments with varying opening sizes, and an unharvested treatment, were applied. Treatment units are 30 ha in size. The project incorporates 28 research studies that are investigating the effects of opening size and stand structure on different aspects of ecosystem structure and function. Some of these studies are experimental in nature, while others are descriptive. Not all have yet been implemented. For the most part, the studies are independent of one another, although a workshop was held in 1994 to integrate and co-ordinate them, with the goal of strengthening the overall project.

Key question

- What is the effect of opening size on ecosystem structure and function?

Experimental design

- replicates of five treatments: (i) control (no tree removal), (ii) single tree selection, (iii) 0.1 ha openings, (iv) 1 ha openings, and (v) 10 ha openings;
- all treatments are applied to 30 ha cutblocks, and the same total volume and area is harvested in each (except the control);

- all treatments were harvested in same year (i.e., no temporal replication);
- some soil disturbance or micro-site treatments will also be applied in some plots.

Identified problems

- None mentioned.

Identified benefits (potential)

- contribute to greater understanding and thus better management of high-elevation (ESSFwc2) forests;
- help answer the questions of: (i) where should we cut? (ii) how should we cut? and (iii) how much should we cut? Answers to these questions will help managers determine optimum spatial patterns, silvicultural treatments, and rotation lengths in the ESSF biogeoclimatic zone.

Other things we can learn from this example

- The report summarizing the workshop proceedings refers to “adaptive management” as the synthesis of data from routine inventory and monitoring and from literature reviews, and the application of this information to improve management. Apparently, it does not include the deliberate design of management activities to generate information. This highlights the necessity of developing and promoting a consistent interpretation of adaptive management, which recognizes the value of deliberate management experiments.

11. Date Creek Silvicultural Systems Project

Coates et al. (1995).

Background

Date Creek lies in the transitional coast-interior forests of northern British Columbia. During the last 20 years, clearcutting has been the dominant harvesting method in northwestern British Columbia. The method has become controversial, and managers (and the public) are looking for alternatives. The Date Creek project is comparing clearcutting with several partial-cutting techniques in old-growth and mature second-growth forests within the Interior Cedar–Hemlock (ICH) biogeoclimatic zone. Treatments are being applied across different states of soil moisture, in two age classes, in units 20 ha in size. The study examines the impacts of the various harvesting methods on many ecosystem components including water, wildlife, plants, and soils, as well as studying economic consequences of the alternatives.

Key questions

- What is the effect of different levels and patterns of partial cutting on a variety of indicators of productivity and biological diversity?

Experimental design

- specific treatments included: clearcutting with retention of scattered deciduous trees, a heavy removal (60%) partial cut, a light removal (30%) partial cut, and no harvesting. The partial cuts removed single trees and small groups of trees.

- treatments were applied across different age classes (350 and 140 years old) and different soil moisture regimes (mesic to subhygric);
- treatments have created distinct differences in stand structure both between and within treatments.

Identified problems

- organizing regional research teams with district operational requirements;
- dealing with details such as how to mark trees for removal, getting Pre-harvest Silvicultural Prescriptions (PHSPs) prepared and approved in timely fashion;
- agreeing on specific treatments and how to accomplish those treatments;
- involving First Nations, respecting land and resource concerns;
- co-ordinating and scheduling so many projects;
- installing field camps to reduce logistical problems of travel time/distance.

Identified benefits

- interdisciplinary co-operation has improved the research program and created "synergy" among researchers;
- increased Regional to District communication within the B.C. Forest Service;
- improved inventories for ecosystem components;
- encouraged broad, cross-process thinking concerning ecological questions;
- increased awareness of local industry and the public regarding alternative silvicultural methods;
- will increase comparative knowledge of systems;
- will increase knowledge on ecosystem function.

Factors that contributed to successful implementation

- effective co-ordination by operations co-ordinator;
- flexibility of small business program — allowed B.C. Forest Service to set design as condition of harvest;
- researchers all from the same region, thus lots of contact and opportunity for discussion to reduce confusion;
- First Nations support basic research direction;
- support of small business program;
- creative, energetic research team with upper-level support and budgetary support.

ADAPTIVE MANAGEMENT IN THE U.S. FOREST SERVICE

Adaptive management has been formally adopted by the U.S. Forest Service, as part of "Ecosystem Management." The Forest Ecosystem Management Team (FEMAT) Report (1993) recommends the use of adaptive management, as does the Eastside Ecosystem Management Project. However, both examples (summarized below) are still in the planning stages; as yet, no management experiments have been implemented. There are also other examples of individual management experiments that have been implemented (e.g., DEMO project described by White et al. 1994; A. Horton, USDA Forest Service., pers. comm., 1995).

12. Eastside Ecosystem Management Project

Eastside Ecosystem Management Project (Science Integration Team) (1994).

Overview

The Eastside Ecosystem Management Project is an initiative to develop an ecosystem management approach to guide the assessment, planning, and management of forest, rangeland, and aquatic systems on federally administered land within the Interior Columbia River basin in Washington State. Adaptive management is explicitly recognized as one component of ecosystem management. The proposed “scientific framework for ecosystem management” states that goals and objectives should be clearly articulated, and combined to form testable hypotheses. Adaptive management is seen as a way of assessing how well specific objectives achieve desired goals. Hypotheses would be tested in a management setting using scientific methods of experimental design and analysis.

Simulation models that integrate biophysical and social processes are recommended as a tool for synthesizing knowledge, identifying gaps in knowledge, identifying links between system components, and exploring alternative scenarios. Information gained through adaptive management would be used to update and improve these models.

The framework explicitly acknowledges that some events are unexpected and unpredictable, and recommends adaptive management as a way of increasing understanding and thus improving predictability. It does not, however, explain or illustrate how adaptive management would be implemented. As yet, the framework has not been applied and there are no examples of management experiments.

13. Adaptive Management Areas in the U.S. Pacific Northwest

FEMAT (1993); B. Bormann (USDA Forest Service, pers. comm., 1995); J. Henshaw (USDA Forest Service, pers. comm., 1995).

Overview

Adaptive management is explicitly recognized as a key component of the President’s Forest Plan for federal lands in the U.S. Pacific Northwest. The Forest Ecosystem Management Team (FEMAT) identified adaptive management as a key component of ecosystem management, and recommended the designation of specific, discrete “Adaptive Management Areas” (AMAs). Subsequently, a system of 10 AMAs was formally created by the “Record of Decision” (ROD).

The AMAs are intended to provide a geographic focus for innovation and experimentation. In these areas, management agencies are expected to develop and test a variety of approaches for achieving ecological, economic, and social goals. Plans must include clearly articulated objectives as well as anticipated outcomes of different management treatments. Scientists, managers, and members of the public all would be involved in evaluating experimental outcomes and designing further experiments. Monitoring is expected to be more extensive in AMAs than elsewhere. Monitoring programs should focus on policy needs and be sufficiently sensitive to detect ecologically important changes at all relevant spatial scales. Information gained in management experiments in AMAs must be disseminated to managers of forests outside the AMAs. In addition, while adaptive management is focused in AMAs, it should also occur in forests outside the AMAs.

Ten AMAs, ranging in size from 92 000 to 500 000 acres, have been designated. They were deliberately selected to represent collectively a range of physiography, technical challenges, ownership, objectives, biological challenges, and level of human disturbance; they should not be considered as replicates. All occur within the range of the Northern Spotted Owl; there are two in northern California, four in Oregon, and four in Washington State.

The standards and guidelines defined in the ROD are restrictive and allow little flexibility for experimentation. AMAs were conceived as areas where these tight restrictions could be loosened in order to increase the rate of learning, although restrictions do still apply.

Management experiments will actively involve managers, scientists, and members of the public. Together they will develop testable treatments. Scientists will be responsible for designing experiments and analyses to allow efficient learning. Managers will be responsible for implementing and monitoring treatments. Managers, researchers, and members of the public will develop hypotheses and analyze outcomes. Each AMA has a co-ordinator, and each co-ordinator is linked to a scientist who acts as a resource person. This differs from Experimental Forests, where managers sometimes perceived that scientists controlled activities. Research and management branches of the U.S. Forest Service each allocate a portion of their budgets to the AMAs.

Example: Applegate AMA

In the Applegate AMA in southern Oregon, landscape pattern and stand structure are shaped by fire. Managers are deliberately manipulating forests to recreate the structure generated by the fire regime that was characteristic of the area before human intervention. They are testing a number of alternative hypotheses, while still meeting objectives for timber production.

Example: North Coast physiographic province

A pilot project in the North Coast physiographic province will examine the effects of tree plantations in riparian areas. Key assumptions underlying knowledge and goals will be tested in management experiments, at three different scales: (i) the entire physiographic province (multiple watersheds), (ii) the AMA (single, large watersheds), and (iii) one plantation. For example, at the largest scale, “packages” of treatments are compared. Different sectors of the public would identify with different packages, one of which would include the standards specified in the ROD. At the medium scale, different practices within a given package would be compared.

APPENDIX 2 Adaptive Policy Design for Forest Management in British Columbia

Walters, C.J. 1995. Written submission to Adaptive Management Workshop.

Forest management in British Columbia may soon undergo major changes in response to the Forest Practices Code and other initiatives related to environmental protection, sustainable harvesting, and maintenance of biodiversity. In this new policy environment, it is fair to say that there are no longer any reliable standard operating procedures and decision rules, so that every management decision and initiative should be viewed in some sense an experiment with highly uncertain outcomes in terms of new performance measures like biodiversity. This situation has been widely acknowledged, and there is much interest in designing a so-called "adaptive management" approach to testing new policy initiatives. As one of the originators of this approach, perhaps I can offer some suggestions about how to make it work, based on my experience in fisheries and watershed management.

The term "adaptive management" was first introduced to the natural resources literature by Ray Hilborn and me in 1976, in a fisheries paper that discussed how scientific research conducted separately from management was not producing useful predictions for fisheries managers about the consequences of management initiatives that would take fish populations into domains of abundance for which there was no historical data nor experience to help guide the development of predictions. Books by C.S. Holling (1978) and me (1986) further expanded the idea of treating natural resource management as deliberate experimentation, and a book by Kai Lee (1994) has brought further broad attention to the concept. As these developments proceeded, the term adaptive management came into wide use by natural resource managers, often in reference to (and justification for) trial-and-error or monitor-and-correct management schemes that just represent new labels for traditional ways of doing management (and that we would not consider to be sound adaptive management).

In the following paragraphs, I try to provide a commentary about how to design an adaptive management program for B.C. forests. This program would look very different from a traditional trial-and-error approach. In particular, it would begin with a careful and explicit analysis of policy options and admission of major uncertainties, and this analysis would be used as a basis for restructuring management over the entire operable forest of B.C., in full recognition of how uncertain we are about the future of every bit of that forest.

It is sometimes said that adaptive management is not appropriate to forestry, where very long response times make it difficult to learn by doing, compared to other resources like fisheries. This is nonsense. Forest responses will occur on many time scales, permitting at least some types of corrective learning quite soon. And whatever the delay in learning, management must somehow go on. Eventually, wise experimental decisions will help to guide long-term husbandry of the resource, and an experimental approach now will at least prevent broad application of any single policy that might not work and would preclude options for change in the future. If "forests are forever," as our industry and government assert, there should be no fear of planning for the long term.

Start by defining policy options and policy performance measures

Adaptive management is about dealing with uncertainty. But if you begin a policy design process by trying to identify scientific uncertainties about any managed ecosystem, that process will fail simply because there are literally an infinite number of such uncertainties. Adaptive policy design has to begin with the observation that you will/must somehow proceed with management in any case, so the issue is not uncertainty per se but rather what the management options are and how uncertain you are about the consequences of these specific options. So you have to begin an adaptive/experimental design process with at least an initial layout of the strategic options, and explicit statements about how you will/would decide which is best in terms of specific policy performance measures.

Policy options vary widely in space/time scale of implementation and impact. To insure that policy design does not become impossibly complex, it is wise to start the option identification process by defining a useful basic scale for treatment comparison, this scale most likely being a medium size (50 000–100 000 ha) watershed unit (much smaller units are not managed for locally sustainable production, and much larger ones will have highly heterogeneous forest management practices and options within them.) Recognize in identifying this nominal scale for initial discussions that it will be possible later to deal with other scales by using concepts of nested experimental design (testing smaller-scale treatments within each larger scale experimental unit), and that later analysis of uncertainties may force you to revise/modify the initial focus scale considerably.

As I understand forest policy problems, there are four main policy components that might form the basis for long-term experimental comparisons:

1. Spatial harvest scheduling/pattern — cut block sizes, corridor patterns, buffer strips, access development management.
2. Harvesting methods and transport of product — selective versus clear cut, small-scale methods such as cat/horse logging versus large-scale industrial methods, etc.
3. Silvicultural treatments — site preparation methods, species/type diversity in restocking and brush control, pre-commercial and commercial thinning policies.
4. Watershed restoration measures — slope and road management, restoration of stream channel and riparian zone integrity.

When you examine these in experimental policy planning, your first priority should be to determine just how much flexibility/range of options is really feasible in each policy category. Look not for a best method, but rather for “untested opportunity” to evaluate new methods. It will likely be worthwhile and necessary to seek strong input from industry/environmental stakeholders in this step of the policy development (see last section below).

Discussion of policy options will quickly reveal key indicators/performance measures by which the options could be compared or ranked. Such measures would be the basic experimental response measurement set for an adaptive management program. Note that this set is likely go well beyond standard biological and physical performance measures such as timber yield and biological diversity; it will very likely have economic performance measures as well, such as cost and profitability of harvesting and total job creation. Indeed, some of the largest uncertainties may well be about the economic performance of alternative timber harvesting systems.

Identify major uncertainties by trying to predict the comparative outcomes of policy alternatives

After identifying a candidate set of policy options/prescriptions/strategies for further evaluation, the next step in adaptive policy design is to test or challenge current understanding of the consequences that would follow from each option, by trying to predict what it would do to the performance measures. This attempt to make predictions should be carried out in as thorough and careful a manner as possible, most likely using various formal models and simulations, so that in the end it is easy to pinpoint those predictions/outcomes that are truly uncertain (and which would not be resolved simply by further simple research and/or modelling).

This step should be focused explicitly on, and restricted to, prediction of comparative differences in performance measures between management treatments. It is not necessary to pretend that all scientifically interesting dynamics must be predicted, or that absolute predictions of change over time can be made in the face of unpredictable climatic and economic changes. A focus on questions like “will policy A do better than policy B in terms of performance measure C?” can go a long way toward avoiding a lot of time (and research) on uncertainties that either can never be resolved or are not directly relevant to the future of forest management.

Predictions, and the uncertainties about them, should be defined as explicit temporal projections (trajectories of response), not as simpler before-after or static comparisons. This demand for clarity in definition of response time scales will be important later in identification and scheduling of priorities for experimental monitoring.

Use policy-screening models to define a good set of policy treatments

Many potential policies or management regimes have hidden pitfalls or deleterious cumulative impacts. For example, “green-up” restrictions on cutting around recent cutblocks can drive the harvest scheduling system to spread harvesting, road development, and a variety of other impacts far more widely over the landscape than would be ecologically desirable. In contrast, some relatively expensive environmental management policies that are often suggested, like putting logging roads to sleep, might have little real impact on important performance measures. Often such cumulative effects and ineffective policies can be made obvious by relatively simple modelling exercises and management gaming procedures.

The process of weeding out policies that are not worth further testing has come to be called “policy screening.” Screening is particularly important in forest management situations where policy testing may commit relatively large parts of the landscape to particular regimes for very long periods of time; the cumulative cost of stupid commitments can be very large, justifying a substantial “front-end” investment in careful screening.

Partition the landscape into experimental units at scales appropriate to the uncertainties

If you begin with the view that there is no standard best policy or operating procedure any more for B.C. forests, so that all management prescriptions are to be treated as experimental, then it will be relatively easy to lay out experimental designs over the entire forest landscape (such that every point on the landscape is assigned to one experimental regime or other). This is a very different view of adaptive management that the simplistic notion of "pilot testing" that has been applied to particular, short-term experimental questions about the efficacy of management measures such as site preparation treatments in silviculture.

Treating the whole landscape as experimental units reduces the experimental design problem to two tractable questions: (1) what proportion of the land to devote to each of the basic treatment regimes identified to have major uncertainty and potential opportunity for long-term improvement, and (2) how large to make each experiment unit (an experimental unit is an are subject to one experimental policy option or regime). Obviously there is a trade-off here. The smaller the experimental units, the more replication and variety of policy options that can be tested. But the smaller the unit, the higher is the risk that the results (at least in terms of animal ecology performance measures) will be dominated by edge effects and/or will not reveal effects of large-scale spatial processes. For example, simulations of proposed conservation areas for spotted owls (SOCA strategies) indicate that fragmentation of the landscape into conservation areas with a few birds each could dramatically increase the risk of extinction for the species (by reducing large-scale dispersal success of juvenile birds), so that it might be better to have a single very large conservation area near the U.S. border. This is a case where there is only one opportunity or experimental unit of the scale needed to test one of the policy options (single large area near border), and to proceed with experimental comparison in such cases means accepting a substantial risk that the experimental results will not be interpretable.

A critical requirement for good experimental design and long-term evaluation is to insist on replication of treatments wherever physically possible. When you set up an unreplicated experiment, for financial or monitoring convenience or because there is only one treatment opportunity, the outcome is almost always to leave a legacy of uncertainty almost as bad as what you started with. An unreplicated experiment demonstrates *only* that experimental units are different from one another (and any two pieces of our landscape are always going to be different from one another), not that the difference is due to any treatment difference that you may have applied. You can be sure that critics of a particular policy will make this basic scientific point some day, when you try to argue that the difference was due to policy. The bottom line of this argument is a working recommendation: experimental units should be made as small as possible, subject to constraints set by the minimum scale needed to "see" effects of all critical processes that lead to policy uncertainty, so as to maximize the number of replicate applications of each treatment. A side benefit of this approach to setting unit size is to minimize the risk of being trapped over large areas by irreversible policy impacts, i.e., the risk of "putting all your eggs in one basket."

It may not be possible to define or agree upon an acceptable minimum size for experimental units aimed at testing certain policy options such as provide migration corridors for birds and mammals. This is because no matter how large an area you examine, there will be some ecological processes (like bird migration) that transcend this scale. Should unresolvable debates arise in the design process at this point, you may find it necessary to make experimental unit itself a design variable (i.e., test some policy prescriptions on a set of experimental units that range widely in size.)

A few other concepts from experimental design should enter the analysis at this point. Make use whenever possible of split-plot and nested experimental designs to increase the richness of policy combinations to be compared. Try to avoid complex factorial designs that consume/commit large numbers of experimental units to rigid treatment regimes, but do not be afraid to use such designs if there is great uncertainty about the "interaction effects" of various combinations of policy treatments. Where there is great uncertainty about the time pattern of treatment response due to effects of uncontrollable environmental factors (climate change over time), consider using a "staircase" design where the same experimental treatment is initiated in different years, with treatment starting on only one or two units each year.

Plan to monitor only key responses, at a variety of time/space scales

If scientists are asked to develop monitoring programs, rather than focusing the experimental monitoring on key policy performance measures, you will almost certainly end up with an impossibly cumbersome and expensive monitoring program over the experimental treatment set. The Carnation Creek experiment on Vancouver Island is a good example of this problem. Instead of comparing various forest management treatments in terms of their overall impact on fish populations over a representative set of watersheds, this unreplicated experiment instead involved detailed monitoring of a wide variety of hydrological and ecological variables in a single watershed. The reason for detailed monitoring is simple: without it, scientists often cannot provide credible mechanistic explanations for observed treatment responses.

The business of scientists is to seek understanding, and this requires detailed measurement that is often in direct conflict with the business of management, which is to seek useful policy comparison whether or not the comparative differences can be explained in detail. In designing large-scale experiments, it is important to strike a balance between these interests. There is value in some detailed monitoring, since understanding is usually a good basis for modifying policy later and for identifying imaginative new policy alternatives. But the first and foremost priority has got to be good, well- replicated experimental design and direct measurement of policy measure responses. My recommendation is to keep the basic monitoring set over all experimental areas as small as possible, and make sure that simple and direct performance measures have top priority. Remember also that responses will occur at a variety of time scales, so that some expensive monitoring programs can be deferred or conducted at leisure.

Once you have devised an overall experimental plan with strongly contrasting policy treatments, you can be sure that scientific investigators will flock to this opportunity to do comparative, large-scale research on basic issues that interest them. In other words, it will not be necessary to accommodate all measurements of scientific process interest in the initial experimental design, or to compromise that design by reducing the number of experimental units just so more can be measured on each unit.

Use AEAM workshop modelling to enhance communication and stakeholder involvement in the policy development process

The whole process of policy identification, experimental design, and implementation will ultimately require, and will be much strengthened by imaginative input from the whole range of stakeholders who can influence future management policy. It is important to avoid the problem that occurred in the Commission on the Resources and Environment (CORE) process, where distrust (and misinformation campaigns) developed because at least some stakeholders felt that government professionals were controlling the process by providing most of the data, technical analysis, and policy option formulation and evaluation. You should try to develop policy comparison models that invite data input, scrutiny, review, and policy gaming by a wide variety of people, and invite those people to participate actively in the model development process.

There is a well-established process or protocol for involving multiple stakeholders in policy development modelling; this is the Adaptive Environmental Assessment and Modelling (AEAM) process. AEAM workshops can be used to structure and obtain stakeholder involvement in all of the design development steps mentioned above. The AEAM process usually begins with multi-stakeholder involvement in a "scoping workshop" to identify basic policy options, performance measures, and sub- models needed for further analysis and policy screening. Development of these sub models and a game-playing interface then proceeds in a series of smaller, more focused workshops. Final model gaming, experimental design testing, and consensus building about design options may then take place in a few further workshops.

AEAM modelling "shells" have already been developed for forest management analysis, linking GIS forest information with a user interface that invites information review and policy gaming. Tim Webb (ESSA) has perhaps the best shell so far, developed through his work for Ecotrust on Clayoquot Sound. There is a simpler University of British Columbia shell (Latrobe model). Scott Akenhead has developed more elaborate shells for use in the CORE process. B.C. Ministry of Forests has initiatives in this area also. So you should be able to proceed fairly quickly with an AEAM process, with the aim of developing preliminary design concepts and options for some demonstration watershed units within the next year or two.

APPENDIX 3 Lessons for Implementing Adaptive Management

Leamann and Stanley (1993) describe an experimental management program for two Pacific ocean perch stocks off British Columbia One program involved a 5-year period of specified overfishing on a stock off southwest Vancouver Island, and the other a five-year period of unspecified overfishing, followed by an equivalent period of closure, on a stock near Dixon Entrance. Both experiments were designed and conducted with commercial industry participation, to test various hypotheses (e.g., about stock dynamics, productivity). A number of problems were encountered in implementing the strategy as planned. Based on this experience, Leamann and Stanley provided the following suggestions for others undertaking management experiments.

1. There should be a clear statement of objectives at the outset. These objectives should be endorsed by all levels of participants.
2. Indices and criteria for evaluation of results should also be agreed upon during planning stages. The interpretation that will be placed on specific types of changes to indices must be agreed upon at the outset. There should also be agreement on what actions will be taken in response to changes in indices, prior to the time when these actions are required. In addition, a mechanism should be in place to guarantee that information will be gathered from individuals who cease to participate in the experiments.
3. Agreement on the forum in which the results of experiments will be interpreted, as well as when and by whom evaluation decisions will be made, should be gained at the outset. Planning of future actions, and even the conduct of ongoing experiments, will be most effective within a single management and evaluation forum.
4. There should be frequent reviews with all participants during the course of the experiment. The reviews should not only examine experimental progress but should also be used to re-confirm objectives, as well as the design and evaluation criteria.
5. A commitment from all levels of industry and government to the process and the time frame of the experiments, and on how to use the results that may affect the design of future management programs, should be acquired. Institutional impatience should not compromise the proper conduct of experiments.
6. Participants should be made aware of potential results, both biological and economic, prior to undertaking the experiments. For one of the experiments described, the economic impacts of removing an unrestricted fishing program, once instituted, were greatly underestimated.

APPENDIX 4 Experimental Design: Classical and Non-classical Approaches as Applied to Adaptive Forest Management

Section 5.3 outlines classical and non-classical approaches to experimental design and analysis of results. The four basic elements of experimental design listed in Section 5.3.1 (null and alternative hypotheses, controls, replication, and allocation of treatments) together with consideration of statistical power and significance levels, must be considered when designing large-scale experiments in forest management. In this Appendix we discuss these elements as they apply to adaptive management. We also briefly explain the concepts underlying Bayesian analysis.

Elements of experimental design

Null hypotheses

The "key questions" discussed in Section 5.2 are what Romesburg (1981) refers to as research hypotheses. In a classical experimental approach, these research hypotheses are refined into statistical test hypotheses, which are usually stated as null hypotheses (H_o). Null hypotheses usually posit that a given treatment has no effect. Researchers then attempt to disprove null hypotheses, thus showing that treatments do indeed cause responses. Popper (1964) argued that there is no such thing as proof in science; that instead, science advances only by disproving proposed explanations for observations.

Alternative hypotheses

Tests to address hypotheses are most effective if they do not treat each as an independent entity, but rather compare them with alternative, competing theories. This approach has been called "multiple hypotheses" (Chamberlin 1897) and "strong inference" (Platt 1964). The formulation of competing hypotheses should occur immediately after null hypotheses have been derived from the "key questions." Devising competing hypotheses that are sharp enough to allow some to be excluded, increases understanding of the system being tested. Increased understanding allows more effective management.

The effective use of competing hypotheses involves (Platt 1964):

- devising alternative hypotheses;
- devising a crucial experiment (or several of them), with alternative possible outcomes, each of which will, as nearly as possible, exclude one or more of the hypotheses;
- carrying out the experiment so as to get a clean result;
- recycling the procedure to refine the possibilities that remain.

While it is a trivial task to generate testable hypotheses, it is much more difficult to develop and priorize those that will provide information relevant to management decisions. Devising the competing hypotheses requires intellectual inventions, creativity, and induction. Representing alternative hypotheses graphically may further help to refine them. The hypotheses must be chosen so as to allow experiments whose outcomes permit exclusions. It is also important to review resulting test hypotheses to ensure that they effectively address critical management issues (see Section 5.2). To be worth testing in management experiments, alternative hypotheses should suggest alternative management activities. For example, Sainsbury et al. (1994) (see Appendix 1) proposed four alternative hypotheses to explain observed changes in fish species composition that followed the introduction of trawl fishing to the North West Shelf of Australia. These different hypotheses suggested three different management solutions. An experimental management strategy was used to discriminate between these alternative hypotheses and reduce uncertainty about the most appropriate management solution.

Sometimes one experiment will not be sufficient to distinguish among all the alternative hypotheses that have been identified. Each alternative hypothesis may require its own separate set of experiments (Romesburg 1981). Explicit statements that identify the full spectrum of competing hypotheses aid in designing crucial experiments and useful sampling schemes. As a simplified example, a key question to be addressed though adaptive management may include the notion that corridors are important features in managed forest landscapes. We may view corridors to be important because they allow seasonal migration of large mammals, or, alternatively, because they allow dispersal of smaller creatures. These competing views can be formulated as null hypotheses, each of which will suggest tests or observations that will exclude one or the other explanation (or both) and thus increase our understanding of the role of corridors. Presumably that increased understanding should allow managers to design more useful corridors. Several other examples described in Appendix 1 also illustrate the effective use of alternative hypotheses (e.g., McAllister and Peterman 1992a; Walters et al. 1992; Gratson et al. 1993; Semel and Sherman 1993).

Unfortunately, the use of alternative hypotheses or alternative models (Connor and Simberloff 1986) is rare in ecology. Because the development of the full spectrum of competing hypotheses, together with experiments that permit exclusion of many, will greatly increase understanding of the system in question, we recommend their use in the process of adaptive management. Alternative hypotheses can also be effectively used to test components of models that aim to increase our understanding or expose gaps in our knowledge of forest systems (Bunnell 1989).

Experimental controls

Hypothesis testing using experimental manipulations requires that background conditions be tightly controlled or that sufficient replication occurs to differentiate between background variation and treatment effects. When background variation is high, the variation attributed to natural variation and experimental error could easily be larger than the variation caused by the imposed treatments. Because we can rarely control background conditions in ecology and management fields, controls must be experimental units that receive no treatment. Ideally, controls should be replicated, just as treatments are replicated. No amount of effort during data collection and analyses can make up for lack of sufficient experimental controls.

Using data collected without control or knowledge of background conditions introduces the problem of incorporating effects of unknown factors into the research. These types of data are collected during mensurative experiments (Hurlbert 1984) where measurements are made at different times or different places, and time or space is the only "treatment." The use of mensurative data must be limited to describing states or processes, or to drawing correlations, without strongly suggesting cause and effect.

Unfortunately, experiments that give reliable knowledge are often more expensive than collection of the less tightly controlled data that are adequate to indicate correlations. Both are useful and can play a role in adaptive management. Since adaptive management aims most often to understand ecosystem processes, however, the use of carefully controlled experiments will be far more useful than the limited analyses allowed by data collected under less rigorous conditions.

Dealing with multiple scales

To be useful, manipulative experiments must be implemented at appropriate scales. In many cases, past manipulative experiments have been implemented at only small scales. Results of small-scale experiments can differ substantially from results of larger-scale experiments, usually because of the emergence of community interactions at larger scales. Effective adaptive management therefore involves careful decisions about appropriate experimental scales.

Designing experiments to deal with processes that cross several spatial and temporal scales will be a challenge. Important processes range from soil interactions operating over less than a square metre, to global atmospheric dynamics that affect establishment, regulation, and disturbance of forests. Computers and Geographic Information Systems (GIS), satellite imagery, and remote sensing techniques are helping to analyze the links between these large- and small-scale processes. Ecosystem responses relevant to forest management can occur over time scales ranging from months to hundreds of years. This presents problems for temporal replication, as well as for monitoring and timely use of results.

Spatial replication

As well as requiring thoughtfully chosen controls, useful experiments require some degree of replication. True replication involves applying a treatment in several experimental units. The ability to provide adequate levels of replication depends on the question addressed and the scales the experiment will encompass. There are two interrelated challenges to providing large replicates in a management setting. First, a high level of natural variability may make it difficult to define true replicates, while also increasing the need for more replicates (to ensure adequate statistical power). Second, when replicates are large, it is more difficult to find enough to implement an experiment with high power. There is a trade-off between the size and the number of replicates in an area. The size and number of replicates may be also be constrained by the costs of monitoring, the costs of foregone revenues, and desire to limit outcomes that are potentially harmful.

Replicating small study areas is relatively simple; finding replicates for large areas may be impossible. Depending on the ecosystem type, the largest experimental unit likely to be effectively replicated is the size of a large watershed or series of adjacent small watersheds (5000–10 000 ha on coastal British Columbia, perhaps up to 100 000 ha in the interior). The appropriate size of experimental units and degree of replication depends on the question addressed, the probability of a undesirable outcome, and how much learning is desired. Walters (Appendix 2) suggested keeping experimental units small (e.g., < 50 000 ha) so that unproven management policies are not applied too widely on the landscape and, more importantly, so that large numbers of replicates are available. In adaptive management, experimental unit size may itself be a variable worth testing.

The "watershed restoration program" in British Columbia (see Appendix 1) (Keeley and Walters 1994) has developed tools for choosing replicates at the watershed scale. Participants in that process adopted a "triplet watershed" approach to replication, where three watersheds would be used to assess a set of treatments. Two watersheds would be assigned the treatments and a third would be used as a control.

Temporal replication

As well as addressing the arrangement of treatments in space, researchers must also address the arrangement of treatments through time. Temporal replicates are necessary for unambiguous interpretation of results when there are potential "time–treatment interactions" (i.e., when the response is affected by the environmental conditions existing when the treatment was applied). Doubts about treatment effects that arise because of temporal confounding could result in the perpetuation of ineffective and expensive activities, or, conversely, could provide an excuse for not implementing an effective but unpopular activity. In some management interventions, suspected confounding of results by time factors has been used by proponents of a favoured management regime to explain away its apparent failures and delay policy changes. For example, a number of years after the start-up of salmon hatcheries in British Columbia, there were declines in numbers of returning adults (Walters and Holling 1990). Some attributed this to the effect of hatchery rearing, and argued for a change in policy. However, proponents of hatchery programs claimed that hatchery stock were simply more susceptible to conditions that were also causing declines in the broader fish population, and argued for retention of hatchery rearing. A staircase approach (Walters and Collie 1989), where the same treatments are initiated in different years, can be used where such time–treatment interactions are a potential problem. However, in forest management, long response times and a limited number of available replicates will pose challenges to achieving temporal replication.

Pseudoreplication

True replication occurs when treatments are applied to more than one experimental unit. Care should be taken to avoid confusing repeated sampling within an experimental unit (pseudoreplication) with true "replication." Simple pseudoreplication occurs when there is no replication of experimental units. There are also other types of pseudoreplication that should be avoided during adaptive management:

(i) Taking measurements over time in the same place and comparing "before and after" as separate experimental units is clearly inappropriate because the conditions before an event will influence the results after that event. Using control sites and comparing differences between control and treatment area before and after a treatment is imposed is a more reliable method of detecting changes at a site over time due to a particular treatment (Stewart-Oaten et al. 1986).

(ii) Pooling of data from several experimental units, or the reverse, treating individual samples as experimental units, is also a type a pseudoreplication. Pooling of samples from separate experimental units is not justified. Experimental units are never perfectly alike, so pooling throws out the information on variability among replicated plots. A measure of that variability is necessary to test differences among treatments.

Applying treatments

When applying treatments to experimental units, Walters (see Appendix 2) suggested avoiding complex factorial designs unless questions about interaction effects are likely important. Split-plot or nested designs increase the potential combinations of treatments that can be compared. The challenge is to develop a split-plot or nested experimental design that will permit clear separation of the effects of as many treatments as possible while controlling for larger-scale processes. Obviously, the more strongly contrasting the management treatments are, the easier it will be to distinguish between alternatives. If time causes confounding, a staircase approach (Walters and Collie 1989) where the same treatments are initiated in different years, could be used.

Economic, social, and political pressures, and aversion to risk can affect the spatial allocation of treatments (e.g., Gratson et al. 1993). Pressure may exist to allocate treatments to areas where they will have the least economic or social impact, or will be least inconvenient. Managers who are averse to risk may be reluctant to impose treatments with potentially negative outcomes in areas with high ecological values. Any biases in treatment allocation can affect statistical analyses of results. Solutions from other disciplines where lack of randomization is frequently a problem (e.g., medicine, social sciences) may be applicable to forest management.

In forest management, treatments often will not be implemented by those who designed the experiment. This introduces the possibility that results may be contaminated by variable and improper implementation (see Section 4.5). This concern was noted by both E. Kurzejeski (Missouri Dept. of Conservation, pers. comm., 1995) and F. Schmiegelow (University of British Columbia, pers. comm., 1995), although neither perceived it to be a big problem. In the AlPac case study (see Appendix 1) some minor adjustments to the original experimental design had to be made because of errors in harvesting the first treatment unit.

Type I and Type II errors

There are four possible outcomes of statistical hypothesis testing, two correct and two incorrect:

1. rejecting the null hypotheses when it is indeed false;
2. failing to reject the null hypotheses when it is true;
3. rejecting the null hypotheses when it is true (known as a Type I error);
4. failing to reject the null hypothesis when it is false (known as a Type II error).

Tests of the null hypotheses should be designed so that both Type I and Type II errors are appropriately minimized (Tanke and Bonham 1985), because, depending on the question, both may result in inappropriate management decisions. As an example, suppose a manager wishes to know whether the number of old-growth associated species in an area is stable or decreasing. If the null hypotheses states that there is no decrease, then a Type I error occurs if the manager concludes that the number of species has decreased when in fact it has remained stable. The manager would decide to protect more old growth. A Type II error occurs if the manager concludes that the number of species is stable when in fact it is decreasing. The manager would take no action and the number of species would continue to decline. Either error is undesirable.

Historically, researchers have sought to minimize the probability of Type I errors by choosing small significance levels (α) during statistical tests, but have given little, if any, consideration to Type II errors. Unfortunately the smaller the significance level (i.e., the probability of Type I error), the larger the probability of committing a Type II error. The probability of committing a Type II error also depends on the degree to which the null hypothesis is false, since the probability of failing to reject a false null is greater for hypotheses that are not "very" false (i.e., that do not greatly differ from actual observations). Fortunately, methods exist to describe Type II errors and determine (and then control) the "power of the test."

The "power of the test" refers to the probability of rejecting the null when it is false (i.e., detecting an effect when one exists). It is calculated as "1-β," where β is the probability of a Type II error. The statistical power of an experiment can be increased by increasing the number of replicates, the contrast between treatments, or the duration of the experiment, or by decreasing the sample variance. Experimenters can also increase power by setting a higher significance level (α). Statistical power analysis can be used both in designing experiments and in ranking alternative designs (e.g., McAllister et al. 1992). It can also indicate when it might be advantageous to reduce sample variance by improving monitoring techniques.

Non-classical approaches: Bayesian analysis

Much of what we call objectivity is an illusion created by agreement on scientific approaches. The types of investigations that people undertake and the structure of their interpretation affects which hypotheses and data are admissible for testing. Each age believes its science is objective; often, the next age refutes this. Bayesian approaches to statistical analysis reflect changes to the classical notion of objectivity. Reckhow (1990) observed that with classical statistics, conclusions are based on the expected behaviour of a large number of repeated samples. While this view is well-suited to studies that involve replicated sampling or a number of subjects, classical statistics does not provide alternatives for experiments that involve few, if any, replicates. Instead, the question has to be reworked to fit classical statistics, is addressed without the aid of quantitative analyses, or is not studied at all. There are many problems in ecology and resource management that are not easily examined using classical statistics.

In classical statistics, scientists start by stating that the treatment of interest has had no effect. They then try to reject that statement (the null hypothesis) on the basis of the data they collect. The collected data are used to estimate the "test statistic." By comparing the test statistic to the distribution of data expected if there were no treatment effect, scientists can address the question: "Given that the null hypothesis is true, what is the probability of obtaining a test statistic as extreme or more extreme that the one observed?" If the probability (i.e., the "P" value) is less than the chosen level of significance, the null hypothesis is rejected. There are two problems with this approach. First, the test statistic is at best indirectly related to the quantity of interest — the truth of the null hypothesis. To reiterate: the "P" value is the probability of observing a particular value of the test statistic, given that the null hypothesis is true; it is not the probability of the null hypothesis *being* true. Thus, classical statistics does not directly answer the question: "Does the treatment have an effect?" Second, typically, the distribution of data that the test statistic is compared to is not constructed from data at hand, but is instead a hypothetical probability function.

In contrast, Bayesian inference, allows scientists to *directly* address the null hypothesis, using information at hand (i.e., "Given the data, what is the probability that the hypothesis is true?"). Bayesian statistics can therefore be used to analyze unreplicated experiments, and to compare the likelihood of several alternative hypotheses (Parma and Deriso 1990; Reckhow 1990; Sainsbury et al. 1994). Managers can update odds on alternative hypotheses as their sum of knowledge of the system changes, and shift policy when the correct model becomes reasonably certain. The probabilities derived can also be used in decision analysis (see Section 6.2). Bayesian inference directly addresses uncertainty in decision-making and facilitates the quantification of expert opinion.

Bayes theorem begins by assigning to each alternative hypothesis a *prior probability* that it is correct. Prior probabilities may be subjective (i.e., based on expert opinion) or calculated from previous studies. Once prior probabilities are assigned, one then calculates the likelihood of obtaining existing data (i.e., those data collected during the experiment), given that the hypothesis is true. Together, the prior probability, the likelihood of the data given the hypothesis, and the sum of the product of the likelihood and prior probability for all alternative hypotheses being considered (P(data) in the expression below), are used to calculate the probability of each hypothesis being true (*posterior probability*).

Bayes theorem is expressed as:

> P(Hypothesis being true given data) = P(Hypothesis being true before knowing data)*P(data given the hypothesis is true)/ P(data), where "P" is the "probability of."

The posterior probabilities can be used in decision analysis to calculate the expected value of different policies (taking into account the value of discriminating between alternative hypotheses, as measured by changes in the posterior probabilities).

The main arguments against the use of the Bayesian approach to statistics are that it is subjective (in assigning prior probabilities) and computationally intensive. However, as noted above, no method of analysis is free from subjectivity. Moreover, the prior probabilities often have less impact than the data on the outcome of the analysis (Carpenter 1990). Computers have eased the computational burden, although it is still necessary to limit the number of hypotheses compared. Other arguments against Bayesian statistics have been raised and refuted (Olson et al. 1990). Perhaps one of the most significant limitations to widespread use of Bayesian inference is simply lack of familiarity — with its advantages, uses, and methods. Carpenter (1990) notes that while graduate students in ecology typically learn classical methods of statistical analysis, they are rarely exposed to Bayesian approaches.